TWO ESSAYS:

ONE

UPON SINGLE VISION WITH TWO EYES;

THE OTHER

ON DEW.

A LETTER

TO THE

RIGHT HON. LLOYD, LORD KENYON

AND

AN ACCOUNT

OF

A FEMALE OF THE WHITE RACE OF MANKIND,
PART OF WHOSE SKIN RESEMBLES THAT OF A NEGRO;

WITH

SOME OBSERVATIONS ON THE CAUSES OF THE DIFFERENCES IN
COLOUR AND FORM BETWEEN THE WHITE AND NEGRO
RACES OF MEN.

BY THE LATE WILLIAM CHARLES WELLS,
M.D. F.R.S. L. & E.

WITH

A MEMOIR OF HIS LIFE,

WRITTEN BY HIMSELF.

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TO

MATTHEW BAILLIE, M.D. F.R.S. L. & E.

THIS VOLUME,

A MEMORIAL OF THEIR COMMON FRIEND,

IS INSCRIBED,

WITH MUCH RESPECT, ESTEEM, AND AFFECTION,

BY THE EDITOR.

A

MEMOIR OF THE LIFE

OF

WILLIAM CHARLES WELLS, M.D.

WRITTEN BY HIMSELF.

Αἰὲν ἀριστεύειν, καὶ ὑπείροχον ἔμμεναι ἄλλων·

MEMOIR.

I WAS born in Charlestown, South Carolina, in May, 1757, being the second son, but fourth child, of Robert and Mary Wells, both natives of Scotland. My mother bore many children afterwards, none of whom lived more than a few years, except one, a daughter, who now resides in London; my brother died about twenty years ago; my two eldest sisters survive.

My father and mother came to Carolina in 1753; but a mercantile scheme which he was then pursuing having failed, he took to the business of a bookseller and bookbinder, to which he had been bred when a youth in Dumfries. He soon afterwards added to these occupations, that of a printer of a newspaper, for which he was well qualified from his previous education, being a good Latin scholar, and particularly well read in history and the belles lettres; he had, besides, studied his own language grammatically, and wrote it with great correctness and purity. In these new employments, both character and ease of circumstances were acquired by him; in consequence of the latter, he was enabled to send my elder brother, nearly five years older than myself, to a considerable grammar-school at Dumfries, which was then kept by a Mr. George Chapman.

I was always my father's favourite, and he, fearing that I should become tainted with the disloyal principles which began immediately after the peace of 1763 to prevail throughout America, obliged me to wear a tartan coat, and a blue Scotch bonnet; hoping, by these means, to make me consider myself a Scotchman. The persecution I hence suffered produced this effect completely.

This object was afterwards promoted by sending me to Dumfries school before I was eleven years old. I remained at it nearly two years and a half, at the expiration of which time, I had finished the course of studies usually pursued there. His correspondent in Scotland then sent me to Edinburgh, in the autumn of 1770. I attended there several of the lower classes of the University, and went also to the school of a drawing master; I mention this latter circumstance, particularly, because it was in this school that I first formed an acquaintance with two of my present most intimate friends, Mr. David Hume, and Mr. William Miller, now better known by the title of Lord Glenlee.

I returned to Carolina in the summer of 1771, and a few months afterwards, was placed as an apprentice with Dr. Alexander Garden, the chief practitioner of physic in Charlestown, and well known to naturalists by his communications to the Royal Society. My manners from my infancy had always been rude and rough, but after I went to Edinburgh, and fell into the company of Mr. Hume and Mr. Miller, and other young men of superior rank to myself, they became considerably softened; but I had always from my earliest boyhood a strong desire to act agreeably to truth. Dr. Garden had been accustomed to apprentices of a very different character, and in consequence, frequently suspected me of falsehood; and upon one of these occasions, he attempted to strike me with his hand, but I eluded the blow. From this time, however, I became in my conduct towards him reserved and indignant, and finding little or no entertainment in the society of the young men of the place, I betook myself seriously to study, and in the course of three years acquired, perhaps, more knowledge, though unassisted, than in any three subsequent years of my life. When I had resided with him somewhat more than three years, the American rebellion first broke out in New England.

My father, whose conduct as the printer of a newspaper had become extremely offensive to the people of Carolina from his constantly maintaining the cause of royalty, found it prudent to leave that country and to return to Great Britain. Soon after he went away, public matters became worse, and I was desired with others to sign a kind of state paper there, "the association," which as it appeared to me to be an open act of rebellion, I positively refused to do. I therefore determined to leave the country also, but my services were now of considerable importance to my master, who was at the same time one of my father's attornies; my mother's brother was also one of his attornies; and these two, along with my elder brother, strongly resisted the execution of my design; but my mother who was a third attorney, a woman of an enthusiastic turn of mind, declared, that the first public act of my life should never disgrace me; she, therefore, in spite of the attempts of the others, sent me off to England about three months after my father had parted from her. I arrived in this country in the autumn of the same year, 1775, and was most kindly received by my father, and applauded by him for my conduct.

In the beginning of the winter of the same year, I went to Edinburgh, and commenced my regular medical education; strengthening at the same time, my former friendships with Mr. Hume and Mr. Miller, with whom indeed I had kept up a correspondence whilst I was in

Carolina. I studied three winters in Edinburgh, and in the course of that time gained a third intimate friend, the present Dr. Robertson Barclay. I passed my preparatory trials for the degree of doctor in medicine in the summer of 1778; but did not at that time completely graduate. In the autumn I returned to London, and attended a course of Dr. William Hunter's lectures, and took instructions in practical anatomy.

Having been about this time offered a surgeoncy in a Scotch regiment, in the service of Holland, to fit myself in some slight degree for it, I became a surgeon's pupil at St. Bartholomew's hospital for three months. The only excuse I can offer for my boldness in accepting this office, is, that I was told by my friend who proposed it to me, that the battalion to which I should belong, did duty in the same garrison with another battalion of the same regiment, and that I should consequently enjoy the assistance of the surgeon to it, a man of considerable experience, but of no school education. On this adventure I embarked early in 1779, and for some time felt myself pleasantly situated in the regiment; but the colonel, who had been promoted to his command, merely from being an officer in the Dutch guards, and bearing a Scotch name, Hamilton, but scarcely able to speak English, soon began to find fault with my conduct, and in consequence once confined me for two days, in the main guard of the garrison, and a second time, for several days, in its prevost, or military prison. This behaviour, it may well be supposed, could not be borne by a high-spirited Englishman. I therefore resigned my commission, and upon the very day of receiving my dismissal from the service, I attacked him openly in the street, and dared him to fight me; he became furious, and ordered a file of musqueteers to seize me, and carry me to prison. Upon the same day he dispatched an express to the Hague, reporting to the Marshal Duke of Brunswick, that he had been openly attacked in the streets by one of the surgeons of his regiment. This was held by the marshal as so violent a breach of discipline, that it was thought the least punishment I should receive, would be confinement for several years in a remote military prison. Fortunately, colonel Hamilton was not the senior officer of the garrison, and two days after, there arrived, in the ordinary course of business at the marshal's office at the Hague, a report from the commanding officer, in which it was stated, that surgeon Wells was no longer an officer in the service. This completely altered the state of affairs, and colonel Hamilton was desired to seek redress for his injury, in some other way than by complaining to the marshal. There is no doubt that the natural warmth of my temper urged me to do, what might have appeared to another person rash, but that I could not have been much in fault is evident from this, that, the present Dr. Storer of Nottingham, and the present Dr. Stewart of Perth, both previous surgeons to the same regiment, and the Rev. Mr. Pearson its chaplain, now residing in the neighbourhood of Chelsea, had all abandoned their situations in the regiment from the treatment they had received from colonel Hamilton.

Immediately afterwards, that is in the beginning of 1780, I went to Leyden, where I remained for about three months, chiefly occupied in preparing a Thesis upon "Cold," a paltry affair, and having no other recommendation, than that its Latin was altogether my own. From thence I returned to Edinburgh, and in the autumn of the same year, published my Thesis, and received the honour of being made Doctor in Medicine. While I was at Edinburgh at this time, I formed a fourth intimate friendship, namely, one with the present Dr. Lister of London.

Carolina had now been lately conquered by the king's troops; in consequence of which, soon after my arrival in London, in the autumn of that year, 1780, my father requested me to go to that country, to look after his affairs, which had been greatly injured during the war; and as he had not been at all satisfied with my brother's conduct of them, who was the only one of the family that was now there. I arrived in Carolina in the beginning of the year 1781, and found his property there much diminished from various causes*; amongst others, the burning of two valuable houses which had not been insured. I still however was able to render him some service. Having accomplished this, I next thought it would be doing a grateful thing to my family, to produce a reconciliation between my father and my brother. I therefore told the latter, that if he would go to England to see my father, I would remain in Carolina till his return, for the purpose of managing his affairs there. He readily accepted of my proposal, and I in consequence became a printer, a bookseller, and a merchant. I had previously become an officer of volunteers, and had been entrusted by some of my father's friends in England, with the management of affairs in Carolina of considerable importance. All these concerns might have been supposed sufficient to occupy the time and attention of a young man of twenty-four (practically unacquainted with any other employment besides that, which arose out of his particular profession). But some insubordination having been shown by a company of volunteers which was composed of the principal merchants of the place, it was judged necessary that the chief delinquents should be tried by a general court martial of militia officers, and I was applied to by the colonel commandant of the militia, who was my particular friend, to conduct the prosecution as Judge Advocate. I, foolishly enough perhaps, consented, and had as my opponents the two principal lawyers in the place, who acted as counsel for the accused. In the course of the trial I suffered considerable obloquy in consequence of their violence; but the natural firmness of my mind, and a consciousness of doing what appeared to me to be right, enabled me to resist all their attempts to browbeat me; and the sentence given by the court martial was altogether in conformity to my advice. Not long after this, orders were received from the commander-in-chief at New York, to evacuate the garrison. Every thing was now to be performed in hurry and bustle, and I immediately began to prepare for my departure.

I embarked in December, 1782, for St. Augustine, in East Florida, carrying with me as much of my brother's moveable property as I could; amongst other things, a printing press, and a considerable quantity of printers' types. When I arrived at St. Augustine, I determined to put up the press there, and print a newspaper. But here a considerable difficulty arose; the press had been easily taken to pieces in Carolina, and I naturally thought that it might be readily put together again; more especially as I had brought with me a regular pressman: but to my surprise he told me that he knew nothing of the matter; that he could work a press as well as any person, when it was put together, but that the putting it together constituted the particular business of a press-joiner. In this dilemma I recollected that there was amongst my brother's books, one entitled "the Printer's Grammar," containing rude cuts of a printer's press. I studied this book for several days with the greatest diligence; and at length by means of the information derived from it, and with the assistance of a common negro carpenter, completely succeeded in my attempt to put the press in working order. Immediately afterwards, I began to publish a weekly newspaper in my brother's name; the first thing of the kind ever attempted in that country. I still however had much leisure upon my hands; (for from the time that I left London, I had scarcely ever read a book, and had always resolved never to exercise my profession except in Great Britain;) I therefore became a captain of volunteers, as some threats had been made by the Americans to attack East Florida, before accounts had been received of the signing of the preliminary articles of peace; I also accepted of the management of a company of young officers, who had agreed to perform plays for the advantage of the poorest loyal refugees from Carolina and Georgia. In the course of my management, a considerable difficulty arose, about finding a person to undertake the character of Lusignan, in Zara. As I had once seen Garrick in this character, about seventeen years before, (the only character indeed, which I had ever seen him perform), I determined to attempt it myself. My success was great. This induced me soon afterwards to appear in the character of Old Norval, in Douglas; and here my success was still greater. I was foolish enough not to stop, but afterwards appeared in the part of Castalio, in the Orphan. My exertions here were mere common place; and I failed still more, in an attempt to appear in comedy.

About this time I received a letter from my father, in which he requested me, now that preliminaries of peace were signed, to go again to Carolina, for the purpose of looking after his affairs. I answered, that I should immediately comply with his desire, though I was confident it would be at considerable risk in some way or other. I accordingly went to Charlestown about Midsummer, 1783, furnished with a flag of truce from General Tonyn, which, in the previous intercourse between the two countries during the war, had always been regarded as a sufficient security against any attempt upon the liberty of the person who bore it, though he might be held indebted to any person in the country to which he was going. Immediately upon my arrival in Charlestown, I was arrested upon a private suit, originating out of a transaction of my brother's. I refused to give bail, on the ground that this would be deserting the security I had obtained by the flag of truce received from General Tonyn. My refusal proved most fortunate. For the night after my committal to prison, a numerous mob assembled before the house of Mr. and Mrs. Harleston, (who had insisted upon my staying with them as their guest, whilst I remained in Carolina,) demanding, that I should be surrendered to them. I had looked forward to an outrage of this kind, and had therefore provided myself with arms, being determined never to surrender myself to a mob. A Mr. Simpson, a young English barrister, the son of the late attorney-general for Carolina, who had never done any thing to excite the animosity of the people of that country, was, notwithstanding, upon the same night seized in the house of a lady with whom he was residing, and flung into the harbour in deep water, where he only escaped death by being able to swim. I remained in prison upwards of three months, during which time I was robbed by another prisoner, and in consequence of my complaining of this, was most grossly abused by the jailor in the common newspaper. Learning that an Irish gentleman, a Mr. Ædanus Bourke, was one of the judges of that country, I immediately complained to him of the jailor's usage, which he directly put a stop to, by a severe letter to him, a copy of which was sent to me. Immediately upon my having been confined, I wrote to General Tonyn, acquainting him with my situation, and saying, I should be ready to undergo any suffering, rather than that his flag of truce should be tarnished while in my possession. I received no positive answer, for he was a very dilatory man, till upwards of two months after my application had been made to him. A government vessel then arrived at Charlestown, bringing a Captain Wyllie as a commissioner from General Tonyn, to demand my release; Captain Wyllie was, at the same time, privately instructed to inform me, that if this could not be obtained in consequence of his interference, I was completely at liberty to regain my freedom, by paying the unjust demand which had been made upon me; this last measure it was at length found necessary to have resort to.

As soon as this affair was terminated, I embarked with Commissioner Wyllie in the vessel which had brought him to Charlestown, and proceeded towards St. Augustine. The master of the vessel was king's pilot for the harbour. This probably made him fool-hardy; for in weather a little windy, but not stormy, he ran his vessel aground upon breakers which had previously occasioned the loss of many vessels. She immediately bulged, and lost her masts, and it was expected that her deck would separate from her ribs, and be carried out to sea, as the tide was now falling. The wind, however, became moderate, and the accident which we dreaded did not happen. With some others of the passengers, I had stripped myself completely naked, and lashed myself to the capstan, in order that I might have something firm to abide by, and not be washed away by the waves. Some hours after this, the tide having begun to turn, and set in towards the harbour, and the evening becoming dusky, it was determined by those who could swim, to make their way through the breakers, as we saw boats waiting for us in smooth water at their edge. Commissioner Wyllie preceded me, and when taken up, told an intimate friend of mine, who had come down in his boat to assist me, that I should certainly be drowned, as I was unable to swim. Shortly after he had left the wreck, I determined upon making the same experiment myself, and with the assistance of a stout sailor got through the breakers, sometimes swimming and sometimes wading. The weather having become still more moderate in the night, those who were left upon the wreck were easily saved the next morning; but in the course of a few hours afterwards, the vessel went entirely to pieces. It may be mentioned here, that the master of the government vessel who had brought us into this situation, was, a few months afterwards, drowned among the same breakers.

In the course of a few months, my brother arrived from England; in consequence of which, I embarked at St. Augustine for Great Britain, i. e. in the month of May, 1784.

During the time I had been in America, which was nearly four years, I completely gave up all study, not even reading the magazines and pamphlets which had been sent me by my father. I have already said in what manner much of my time had been employed; I shall now add, that another considerable portion of it was spent in female society, which I had formerly much neglected. As soon as I returned to London, I began to think seriously of studying my profession, to fit myself for the exercise of it; and in consequence, cultivated the acquaintance of medical persons. In this way I became acquainted with the present Dr. Baillie, and soon after contracted with him an intimate friendship, which now constituted the fifth, and has been the last I have ever formed.

The next spring I spent three months in Paris, more with the view however of seeing the place, than for improvement in my profession.

About Midsummer 1785, I returned to London, and in the autumn, had the name of Dr. Wells affixed upon the door of a lodging which I had hired. During the war my father's affairs had prospered, and at the end of it he regarded himself worth about £20,000. But as soon as peace took place, his principal correspondents became dilatory in their remittances, but were still urgent for additional supplies of goods; he was weak enough to comply; so that when I returned to this country in 1784, I found him considerably embarrassed in his circumstances. He told me, however, that he regarded this embarrassment as only temporary, and therefore urged, that I should exercise my profession in London, expecting, that he should hereafter be enabled to afford me all necessary assistance, though at present, he could give me nothing more than the use of his table. I was obliged, in consequence, to borrow £130 from one of my friends, to enable me to commence my career.

I soon found, after a very trifling experience, that I was a good deal unfit for early success in my profession in London; for I entertained a very high notion of its dignity, and felt a great contempt for most of the apothecaries with whom I became accidentally acquainted; in consequence, I passed several years almost without taking a single fee. I fortunately then was chosen one of the physicians to the Finsbury Dispensary; for I now was furnished with the means of studying medicine practically, and received from the institution a gratuity of £50 annually; some few fees also were the consequence of my appointment; but I had resided in London fully ten years, before my income from every source amounted to £250 per annum. To supply the consequent deficiency, I was frequently obliged to make further loans from my friends, until the whole of my debt amounted to about £600. I think it right in justice to myself however, to mention, that these were my only debts; for I never allowed a tradesman to call for money and go away without it.

About 1795, my professional receipts became equal to my expenditure; agreeably to the rigid, and almost sordid manner, in which it was conducted. In the next five years, I was enabled to pay off a little of my debt.

At the expiration of that time, in 1800, I was suddenly seized with a slight fit of apoplexy. From this, however, I did not recover so far as to be enabled to return to the exercise of my profession, for several months; and I never afterwards regained the complete possession of my memory. I became, too, much more unfit for the pursuit of any difficult train of thought, which was the production of another person. I did not however, as well as I could ascertain, become less equal than I had been, for the pursuit of my own trains of thought; in proof of which, I may perhaps be allowed to say, that in the fourteen years following this illness, I made more literary efforts than I had done, during the whole preceding period of my life. Dreading however another attack of apoplexy, or one of palsy, warnings of which I had almost daily since that time received, I determined to live most abstemiously, and in consequence, took not more food when I was at home (I dined there about four or five times a week) than was sufficient for a child of seven years old, and that consisting of vegetable matter. I was the more induced to adopt this manner of proceeding, as my father and one of his brothers had previously died of apoplexy;

and a younger sister of my own had been attacked with the same disease when in a state of parturition. It was successful as far as the disease of my head was concerned; for I never suffered a second attack of it; but my health became infirm in other respects, and I was seized at different times with several dangerous diseases, having no apparent relation to my great ailment.

I had long meditated making some inquiry into the nature of Dew, which I thought would not occupy me more than a few nights, at the house of one of my friends in the country. I commenced it in the autumn of 1812, but soon found that I had greatly miscalculated the time which it would employ me. I determined however to proceed from the natural steadiness of my disposition, which would never allow me to abandon any pursuit that I had seriously undertaken. I soon found that I was altogether unequal to it; for each night's labour fatigued me so much, that I could not undertake a second for several days after. In the mean time my ancles began to swell in the evening, which I regarded as a mark of general weakness. At length, I became so infirm about the end of 1813, that I was absolutely obliged to give up any further visits to the country.

In the beginning of 1814, a considerable snow having fallen, I could not resist the temptation of going for several evenings to Lincoln's-Inn-Fields, during a very severe frost, in order to repeat and extend some of Mr. Wilson's experiments upon snow. I soon however was obliged to desist. I became breathless on slight motion; and was frequently attacked with palpitation of my heart. My friend, Dr. Lister, became alarmed at my situation, and strongly urged my remaining quiet, as he thought it improbable I should survive more than a few months. Upon receiving this opinion, I set about immediately composing my Essay on Dew, as my papers containing the facts on which my theory was founded, would, after my death, be altogether unintelligible to any person who should look into them. I laboured in consequence for several months with the greatest eagerness and assiduity, fancying, that every page I wrote was something gained from oblivion. Dr. Lister's opinion, however, did not prove altogether correct, for my breathing and palpitation gradually became less; chiefly, as I conceived, from my ceasing to expose myself to those causes which had originally produced them; and when my Essay was published in August, 1814, my health was nearly in the same state as it had been before I had begun to make my experiments, except with regard to the swellings in my feet, which still continued, appearing chiefly in the evening.

Having now got rid of the most urgent effects of my long and laborious pursuit concerning the cause of dew, and the condition of my head being fully as good as at any time since the first attack of apoplexy, I began to take increased liberties with respect to my conduct in life. I therefore, though still adhering most rigidly to my plan of abstinence with regard to food, often exposed myself to considerable bodily fatigue, both by making journies in the country, and taking an unusual degree of exercise in London, to which latter circumstance, I was especially exposed from not keeping a carriage.

In the beginning of the present year, I observed that I frequently, as by an involuntary act, made a sudden and deep inspiration, and in the beginning of June I was several times affected at night with violent pains in my right side, apparently seated in the muscles. When this happened, I always found that I was lying upon my right side, and when I placed myself upon my back, the pains ceased. This was the only situation I could assume, for, ever since I had been affected with palpitations and breathlessness, I found it impossible to lie upon my left side.

About the 10th or 12th of June, I was seized at night with an attack of the pains in my right side. As they did not return, however, I went on the 14th on a visit to Mr. Reid's, at Ewell. At dinner on that day, I was as cheerful as usual, and staid up as late as any of the family. On the following day, I felt no disposition to walk, but at dinner time it was remarked, that I took more than ordinary pains to entertain Mr. Reid and his company, which was a large one. Early in the evening, however, I became languid and drowsy, went to bed several hours before the rest of the family, and slept that night a much longer time than I had been accustomed to do. In the morning I was stupid and languid, but came to town immediately after breakfast. On the same day I informed Dr. Lister of my situation, which he soon began to think required the attendance of Dr. Baillie along with his own. I shall not say any thing further of my ailments, except, that at first I never imagined that they would terminate in hydrothorax.

I shall now attempt to give dates to several events which occurred to me in London.

I think it was in 1790, certainly not later, probably twelve months earlier, that I was appointed a physician to the Finsbury Dispensary; I remained so till about the year 1798*. In November, 1795, I was elected assistant physician to St. Thomas's hospital, and in 1800, I became one of the physicians to it, in which situation I still remain. In November, 1793, I was admitted into the Royal Society of London; in 1814, into that of Edinburgh. About four years ago Dr. Baillie asked me, in the name of the President of the Royal College of Physicians of London, if I had any desire to become a Fellow of it; to which I answered that I had none.

In 1792, my Essay on Vision was published. In the Philosophical Transactions for 1795, appeared my paper on the "Influence which incites the muscles of animals to contract, in Mr. Galvani's Experiments;" in those for 1797, my "Experiments on the Colour of the Blood;" and lastly, in

* Dr. Wells was elected Physician to the Finsbury Dispensary on the 3d of September, 1789, and resigned the office on the 11th of December, 1799. E.

those for 1811, some "Experiments and Observations on Vision." I have already said that in 1814, my "Essay on Dew" appeared. I formerly omitted to mention that Dr. Darwin attacked in his Zoonomia, what I had said upon giddiness*. I immediately answered him in two letters sent to the publisher of the Gentleman's Magazine. I have now referred to every thing which I have published on philosophical subjects. In the second and third volumes of the "Transactions of a Society for the Promotion of Medical and Chirurgical Knowledge," almost every thing that I have published upon medicine is to be found; but these are so numerous, that I shall not particularize their dates. In 1799, was printed by me, rather than published, for it was never exposed to sale, a "Letter to Lord Kenyon." These writings, as well as I can recollect, are the only ones which ever were printed by me with

* In the Essay upon Single Vision with two Eyes.

my name affixed to them; but several others have been given by me to the world without this attendant.

The first of these, and indeed the first thing that ever I wrote for the public, was an account of Mr. Henry Laurens, some time president of the American Congress, which appeared in the form of a letter, under the signature of "Marcus," to the printer of the Public Advertiser, in September, October, or November, 1780. In 1800, I published, in the Gentleman's Magazine, some account of the life of Mr. Anthony Lambert, formerly of Calcutta. In the course of the same year, I published also some account of Mr. Wilson, of Bedford-street, Covent Garden. In the course of the next twelve years, appeared in the same magazine, "Biographical Sketches," also written by me, of the following physicians; Dr. George Fordyce, Dr. David Pitcairn, and Dr. Andrew Marshall. In Carolina, during the years 1780 and 1781, I published many small political things, without attaching my name to them; the principal of which was written at the desire of the commandant of the garrison of Charlestown, the present General Nesbitt Balfour. The cause was the following. Men of rank in that country in the American service, after having been taken prisoners, and sent to their homes under their military paroles, used to make no scruple whatsoever to appear again in arms against the British government. I therefore was desired to show, by an appeal both to military usage, and the nature of the thing itself, that such conduct subjected them to the punishment of death. This paper was held of that importance by the commandant, that he gave orders that its publication in the public newspapers should be frequently repeated; and I think it highly probable, that it was owing to this warning, that General Balfour and Lord Moira thought themselves justified in putting to death a Colonel Haynes, the propriety of whose fate was afterwards a subject of debate in the British House of Commons.

I think it right to say something more particular than I have hitherto done, respecting the clear profits of my profession, the only source of revenue that I have ever enjoyed in London. In 1801, the sixteenth year after I had become a physician in London, they amounted only to £307, in which sum were included about £60, which I had received in the form of salary from St. Thomas's Hospital, and of fees, for the attendance of medical pupils there. The following year my total receipts diminished to £235. They remained in this fluctuating state during the three following years, that is, till 1806 included. During the next six years, they fluctuated between £325 and £455. In 1813, they were £457. In 1814, £441. In 1815, £764. In 1816, £572; but in 1815, I received at one time £210 for giving medical evidence at Exeter. The smallness of these receipts will perhaps appear the more extraordinary, when I say, that during a great part of this time, Dr. Pitcairn, and during the whole of it, Dr. Baillie, often sent patients to me; and made every exertion to promote my interest. But I lived at a considerable distance from them, and was unable, from the want of a carriage, and from various other circumstances, to appear properly as their representative. In spite, however, of this smallness of my income, (which, during almost the whole of the time spoken of, that is, from 1801 to 1816, was rendered still smaller, by my paying most rigidly the income and property tax, and allowing an annuity, for a good many years, of £20 to a female relation,) so rigid was my economy, that, during the few last years of my life, I paid off the whole of the money which I had borrowed, amounting, as was formerly mentioned, to about £600; and when I was taken ill, about three months ago, I had in my desk, for I never kept a banker, nor ever invested any money in the funds, about £350. This sum constituted the greater part of my property. For all my books, my little plate and furniture, probably, though much more valuable to myself, will not be supposed by others worth more than £200. In this estimate, the value of my gold Rumford medal is not included; as the gold is quite pure, it is held to be intrinsically worth fifty guineas.

In the expectation that my life would be prolonged, I had formed various literary projects. One was, and this had often passed rapidly through my mind during the last forty years of my life, to show, that there is a material difference in the manner in which we acquire our ideas of the primary and secondary qualities of matter*. If, after a closer examination of this subject than I had formerly given it, I should have found, that my notions respecting it were just, I should have attempted in treating of it, to imitate, in some slight degree, the inimitable manner employed by Berkeley in his Treatise on Vision. I should then have presented to the Royal Society several papers on vision, the chief of which would have treated of those phenomena of light, which have been denominated by authors coloured shadows, ocular spectra, and by various other titles. In the last place, I should have brought together into one volume, all my publica-

* He made out, in his own hand-writing, during his last illness, a short statement of his opinion upon this subject, which, by his desire, has been put, since his death, into the hands of a philosopher, whose great learning and profound researches into the human mind peculiarly fit him for estimating it justly. E.

tions upon vision; which I would have inscribed to the memory of Robert Wells my father, in gratitude, for the great exertions which he had made to give me the education of a scholar and a gentleman, when in narrow circumstances himself, into which he had fallen, in consequence of the American rebellion.

What I shall next say will no doubt be held very ridiculous. I lived till I was near eleven years old, close upon the harbour of a large sea-port in America, and by this means associated much with blackguard sailor boys. To this I attribute a practice of swearing, of which I have from the time of being a child, been frequently guilty, when my feelings have been agitated, and even sometimes when no excuse of this kind has existed.

My last declaration will relate to the obligations under which I lie to my friends. I have already spoken of my rare good fortune, in having acquired, in the course of my life, five most intimate friends. All of these are still in being, and from all of them I have received, throughout my illness, the warmest proofs of attachment. Two of them, however, have most especially afforded such proofs, Dr. Lister and Dr. Baillie, partly from their residing in London, and partly from the nature of their profession. My obligations to Dr. Lister are extreme. During the whole of my disease, he has visited me constantly twice, and sometimes thrice a day; and during each of these visits, he has conducted himself towards me, with fully as much kindness, as if I had been his brother.

I have likewise to express my very great obligations to two other of my friends, Mr.

James Dunsmure, merchant, in Lothbury, and Mr. Samuel Patrick, of Bartlett's Buildings, surgeon; since, in the whole course of my illness, their attentions to me have been most unremitted, and they have also most generously promised to burthen themselves with the care of my concerns, after my death.

It must not be regarded as an instance of the weakness of an old man's mind, my desiring, that my body may be deposited in Lady Jersey's vault in St. Bride's Church; immediately above that of my mother, and in contact with it, as her's is now placed with respect to that of my father; for it has been my wish, for many years past, that this should be done. I have, indeed, never been desirous to conquer any natural feelings, when their indulgence led to no harm; on the contrary, I have always regarded such an indulgence, as highly conducive to the softening of the original hardness of my character.

August 22d, 1817.

As I fancy that several parts of my character, from various reasons, have been a good deal misunderstood, even by my most intimate friends; I shall relate here, with little regard to method or connection, some circumstances which may tend to illustrate it.

I began to show, even in my earliest childhood, an invincible firmness of mind. When my father, who was a passionate man, beat me for a fault, which I was conscious I had committed, I used to entreat mercy most piteously; but if I believed, that I was in the right, the utmost punishment he could inflict would scarcely ever force a tear from me. When I was at Dumfries school, I had a playfellow, the present Mr. ——, of Edinburgh. He one day called me by some improper name, in consequence of which I beat him, being the stronger of the two. He complained to Mr. Chapman our master, who ordered me to promise, that I would never do the like again. I answered that I could not, for I would certainly beat him if he repeated the offence. Mr. Chapman tried first the effect of corporal punishment upon me; but finding this of no avail, he ordered me to retire to my room, for I was one of his boarders, and forbade the other boys to hold society with me. This happened upon a Saturday, which was at our school a half holiday. On the Monday following, I was summoned to appear in the school, as I thought, for the purpose of being [finally] expelled from it; for, I had determined to submit to this disgrace, rather than to swerve from my former declaration. To my astonishment, however, I found that I was to receive from him the highest commendation. On the Saturday afternoon, my confinement not having been strictly enforced, I was determined to break through it, and to go into the neighbouring country with some of my playfellows. There was before the school a considerable area, in which, while I was proceeding to join my playfellows, I met a blind beggar, who appeared to me to have lost his way. The other boys had passed him without attending to him. I went up to him, and finding my conjecture to be right, took him by the hand, and led him to the house to which he was desirous of going. My master was at his window, and saw this. On the Monday he mentioned it to the whole school, and received me back into it, with great commendation of my conduct, without making any reference to my former expulsion.

My father was a man of great sobriety himself, and restricted me, while I was a boy, from drinking any thing but water; and I never, in any posterior part of my life, have had the least desire to taste any stronger liquor, except in compliance with the ordinary customs of society. In 1782, I became president of a club in Florida, and agreeably to the custom of the country, thought it necessary to make my subjects intoxicated. In this attempt, I necessarily became somewhat intoxicated myself, but still in a less degree than the others, from proceeding more cautiously. During the other six days of the week, though living constantly in society, I drank nothing but water, nor did I ever afterwards, even before my health became infirm in 1800, desert this practice, except I was in society.

My father, though naturally a passionate man, in all the ordinary affairs of life conducted himself with the greatest prudence, except in the case already mentioned, when he was induced, by too great ease of temper, to swerve from it. My mother was much his inferior in point of common sense, and had a strong tendency to act a little romantically.

I resembled them both, not only in person but disposition; and, in consequence of my resemblance to my mother in this latter circumstance, began early to show signs of a certain waywardness of disposition.

When I was a boy at Dumfries school, I used to wander on foot during the autumnal holidays through the country, without any fixed object. In one of these rovings, being then in the twelfth year of my age, I went to call upon a friend of my father's, without any other clothes than those which I had upon me. The following morning, I thought my shirt looked dirty, and therefore determined to wash it myself. I chose, as a place fit for this purpose, a little meadow on the side of the river* *Milk*, which was sheltered by a high bank behind me. Having done the business in the best manner I could, without any assistance from soap, I placed my shirt upon the grass for the purpose of drying it, and laid myself in the mean time in the sunshine, upon another piece of dry grass in the neighbourhood. When my shirt was dry, I put it on, and returned to my friends. In the course of the night,

* This, I am told, is a rivulet rather than a river. E.

I was seized with a considerable degree of fever, and in the morning my face, and the parts of my body which had been exposed to the sun, became considerably red and swollen.

About a twelvemonth afterwards, I received an invitation by letter from a school chum, to visit him at his father's, who lived in Galloway, about 31 miles from Dumfries. I showed this letter to Mr. Chapman, and requested money from him, to enable me to make the visit. He most properly refused to give any, upon the ground, that the invitation had proceeded only from a boy. I thought differently, however, and, taking advantage of his absence, began my journey two days after, without a halfpenny in my pocket, and with no other clothes than I wore, as I had determined to return to Dumfries the following day. My friend's father, whose name was Macmurdo, had lived many years in Virginia as a merchant, and when he returned to Scotland, brought with him a wife, who was a native of the former country. They received me most kindly, no doubt somewhat influenced by my having been born in America, and retained me as their guest for upwards of a month; supplying, amongst my other deficiencies, that of raiment. At the expiration of the holidays, they sent me in a post-chaise to Dumfries, with a part of their own family.

In my journey to Mr. Macmurdo's house, which I accomplished in eleven hours, I had no food but hips and blackberries, and a little milk, which a cottager would sometimes give me when I asked for a little water to drink.

My temper was naturally irritable, and in small differences which have occurred in society, particularly in my youth, passionate and violent. But I must, in justice to myself, say, in the first place, I have not shown any considerable instance of this kind for nearly twenty years; and in the second, that I did never show one, even before that time, in any matter of consequence, or when I had any respect for the person with whom I differed. In confirmation of both these remarks, I shall mention, first, that I have never had the smallest difference with any one of my five most intimate friends; and secondly, that I have borne the grossest insult, when it was unmanly to take immediate notice of it.

From the time of the murder of the princess of Lamballe, I foresaw the ruin of all civilized society in France, and dreaded a similar ruin of all civilized society in Europe. I have never, therefore, been able to hear, with the least patience, any serious defence of the conduct of the French; and have always attributed such a defence to incurable folly, self-interest, or madness. In all points of domestic politics, I have kept myself free from personal influence, by never seeking the acquaintance of any person of the least influence in the country. By principle I am a constitutional Tory; but my manners, I should think, would lead most persons to regard me a republican.

August 28, 1817.

Dr. Wells, from a very early period in his illness, looked forward to a fatal termination of it, and employed himself in arranging his affairs with the utmost self-possession and diligence, until he had settled, with great exactness, every thing which he thought important. From the 8th of August, his physicians, as well as himself, abandoned all hopes of his recovery. He died in the evening of the 18th of September.

ADVERTISEMENT.

The Memoir, with the omission of an anecdote, which might have given pain to a family with which the author had been on terms of great intimacy, and of a name and designation, which it was believed the very respectable person referred to might wish to be suppressed, and with a very slight alteration in a very few expressions, is precisely as it was left by the author. He dictated it to his friend Mr. Patrick at intervals during his illness, after he had lost all hope of recovery, and while he was uncertain whether he should live to finish it, and when he was too feeble to speak long, or to write much. It must be considered a proof of extraordinary composure and vigour of mind in such circumstances.

The writings of the author, which have been selected for publication with this Memoir, either as the most interesting in themselves, or as affording the best exhibition of his character and talents, are, an Essay upon Single Vision with two Eyes, and an Essay upon Dew; a Letter to the Right Honourable Lloyd, Lord Kenyon, relative to some conduct of the College of Physicians of London, posterior to the decision of the Court of King's Bench, in the case of Dr. Stanger, and containing observations on a principal ground of that decision; and an Account of a Female of the White Race of Mankind, part of whose Skin resembles that of a Negro; with some observations on the causes of the differences in colour and form between the white and negro races of men. The last of these writings was read before the Royal Society in 1813, but was never printed until now. It was put by the author into the hands of the editor, with an express permission to publish it, and no alteration has been made in it, besides a very slight one of expression, in a few places, which its being presented to the public, instead of being addressed to a philosophical society, rendered necessary.

All his other works, whether philosophical, literary, or medical, (excepting only those of a political nature, which are mentioned in the Memoir, and to which no more particular reference could be made than what is made in it,) are enumerated in the following list, in order that they may be more generally known and more easily referred to.

Two letters, in reply to Dr. Darwin's remarks, in his "Zoonomia," upon what Dr. Wells had written in his "Essay upon single Vision with two Eyes," on the apparent rotation of bodies, which takes place during the giddiness occasioned by turning ourselves quickly and frequently round. These were published in the

Gentleman's Magazine for September and October 1794.

Observations on the influence which incites the muscles to contract in Mr. Galvani's experiments. These were published in the Philosophical Transactions in 1795.

Observations and experiments on the colour of blood. These were published in the Philosophical Transactions in 1797.

Some account of the life of Mr. Anthony Lambert, formerly of Calcutta; and some account of Mr. George Wilson, apothecary, of Bedford-street, Covent Garden. Both these appeared in the Gentleman's Magazine for 1800.

A biographical sketch of Dr. George Fordyce. This appeared in the Gentleman's Magazine for 1802.

A short account of Mr. John Savage, formerly of Charlestown. This appeared in the Gentleman's Magazine in 1804.

A biographical memoir of Dr. David

Pitcairn. This appeared in the Gentleman's Magazine in 1809.

Observations and experiments on Vision. These were published in the Philosophical Transactions in 1811.

A biographical sketch of Dr. Andrew Marshall. This was published in the Gentleman's Magazine in 1813.

An answer to remarks in the Quarterly Review, upon the Essay on Dew. An answer to Mr. Prevost's queries respecting the explanation of Mr. B. Prevost's experiments on Dew. These appeared in Dr. Thomson's Annals of Philosophy for 1815.

A short letter "on the Condensation of Water upon Glass." This was published in Dr. Thomson's Annals of Philosophy for 1816.

The titles of his medical writings are,

1. Observations on Erysipelas.

2. An Instance of an entire want of Hair in the Human Body.

3. Observations on the Dropsy which succeeds Scarlet Fever.

4. A Case of Aneurism of the Aorta, attended with ulceration of the Œsophagus and Windpipe.

5. A Case of Epilepsy and Hemiplegia, apparently produced by a sharp projection from the inner Table of the Skull.

6. A Case of Tetanus, with Observations on the Disease.

7. A Case of Aneurism of the Aorta, communicating with the Pulmonary Artery.

8. A Case of considerable enlargement of the Cæcum and Colon.

9. A Case of an extensive Gangrene of the Cellular Membrane, between the Muscles and Skin of the Neck and Chest.

10. On Rheumatism of the Heart.

11. On the presence of the Red Matter and Serum of the Blood in the Urine of Dropsy, which has not originated in Scarlet Fever.

12. Observations on Pulmonary Consumption and Intermittent Fever, chiefly as Diseases opposed to each other; with an attempt to arrange several other diseases, according to the alliance or opposition which exists between them and one or other of the two former.

These were all published in the second and third volumes of the "Transactions of a Society for the promotion of Medical and Chirurgical Knowledge."

There is also a case of Aphonia Spasmodica described by him, and communicated by Dr. Carmichael Smith, in the second volume of the "Medical Communications."

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AN ESSAY UPON SINGLE VISION WITH TWO EYES:

TOGETHER WITH

EXPERIMENTS

AND

OBSERVATIONS

ON

SEVERAL SUBJECTS IN OPTICS.

AN

ESSAY

UPON

SINGLE VISION WITH TWO EYES.

PART I.

Of the different Opinions concerning single Vision with two Eyes; and principally of those of Dr. Smith and Dr. Reid.

THE end I have chiefly in view, in this Essay, being to offer a new solution of the question, why objects are perceived single with two eyes, I think it incumbent upon me, in the first place, to show, that none of the opinions I have met with upon this subject, can be admitted as just.

These opinions, or such of them, at least, as have gained any considerable reputation, may be reduced into two classes. The first comprehends those of Galen, Alhazen, Rohault, Dr. Briggs, and Sir Isaac Newton, all of whom have regarded the question I have mentioned as equivalent to the following one: Whence comes it, that the mind should be affected with only one perception from two impressions upon the external organs of sight, since either of those impressions is, of itself, sufficient to produce a similar perception? Their universal answer has been: Because the two impressions are united before they are communicated to the mind. And the only difference among these authors, has been with respect to the manner in which such an union takes place. To the second class are to be referred the opinions of those, who hold it as certain, that an object is seen single by both eyes, because it is seen by each of them in the same external place; and who profess to point out some law, or constant rule of vision, from which this sameness of place is to be derived as a necessary consequence. Aguilonius, I believe, first gave this view of the question, which has since been adopted by Dechales, Dr. Porterfield, Dr. Smith of Cambridge, and Dr. Reid of Glasgow.

In opposition to the opinions of the first class, more especially as they have been repeatedly examined by others, I think I need only say, that they must all be considered as mere conjectures, founded upon certain supposed changes in the brain and nerves, the existence of which it is impossible, from the nature of the parts, either to demonstrate, or to refute by experiments; and that no one of them, though admitted to be true, is yet sufficient to explain the phenomena on account of which it was framed.

The opinions of the second class being built, as their authors think, upon experiments and observations, both allow and demand a more accurate investigation. I shall proceed, therefore, to examine such of them as I am acquainted with, beginning with that of Aguilonius; and what I shall observe concerning it will apply also to those of Dechales and Dr. Porterfield, who have done little more than copy what he has said.

If a line be drawn through the point of the mutual intersection of the optic axes, parallel to the interval between the eyes, Aguilonius calls it, from its office, the *horopter*; and if through this line, a plane be made to pass at right angles to that of the optic axes, he names it the *plane of the horopter*. After defining these terms, he asserts, that, by a law of our constitution, all bodies which we see with one glance or look, whatever are their real places, appear to each eye to be situated in this plane. And if this be granted to him, he easily and satisfactorily shows, why some should be seen single with two eyes, and others double. For since, according to a second opinion maintained by him, and not contradicted, I believe, by any other writer upon vision, the two lines of direction, in which an object is seen when we employ both eyes, can meet each other only in one point, it follows, that all bodies which are really situated in the plane of the horopter, must necessarily appear single, as the lines of direction in which any one of them is perceived by the two eyes, coincide in that plane, and no where else; and that all bodies, which are not situated in the plane of the horopter, must as necessarily appear double, since, in this case, the lines of their visible directions intersect each other, either before or after they pass through it*.

Against the truth of this explanation, only one argument need be offered. Were the visible places of all bodies to be contained in the plane of the horopter, these would appear of magnitudes proportional to the angles which they subtend at the eye. A finger, for instance, held near to the face, would seem as large as the part of a remote building it might conceal from the sight. But as this is contrary to experience, the principle from which it is derived,

* Aguilonii Optica, p. 110, 148, 331, 344.

must be rejected, together with all its consequences. To Aguilonius, however, the merit is due, of being the first who so far generalised the phenomena of single and double vision, as to observe, that those objects alone are seen single, which are really situated in the plane of the horopter.

The opinion of Dr. Smith is the next in the order of time. * "If it be asked (says that author) why, in seeing with both eyes, we do not always see double, because of a double sensation, I think it is sufficient to say, that in the ordinary use of our eyes, in which the pictures of an object are constantly painted upon † corresponding places of the retinas, the predominant sense of feeling has originally and constantly informed us that the object is single. By this

* Complete System of Optics. Vol. I. p. 48.

† Dr. Smith gives the following definition of *corresponding points.* "When the optic axes are parallel, or meet in a point, the two middle points of the retinas, or any points which are equally distant from them, and lie on the same sides of them, either towards the right hand or left hand, or upwards or downwards, or in any oblique direction, are called *corresponding points.*" Vol. I. p. 46. According to this definition, points correspond which have a certain agreement in situation. No contradiction is, therefore, implied in this system, by saying, that an object may appear single, though its pictures should fall upon points which do not correspond. Dr. Reid's definition of the same term is very different.

means our idea of its outward place is connected with both those sensations, as is manifest by its appearing in two places when its pictures are not painted upon corresponding places of the retinas; which is only a direct consequence arising from our general habit of seeing." Should any one now inquire whence it is, that, to produce single vision, all men agree in directing their eyes toward the object in such a manner as to receive its pictures upon corresponding points of the retinas, since custom might have connected the sensations of any other two points with the information of its unity from feeling*: This answer may be given in the words of Dr. Smith†: "When we view an object steadily, we have acquired a habit of directing the optic axes to the point in view; because its pictures falling upon the middle points of the retinas, are then distincter than if they fell upon any other places; and since the pictures of the whole object are equal to one another, and are both inverted with respect to the optic axes, it follows that the pictures of any collateral point are painted upon corresponding points of the retinas."

* This objection is made to Dr. Smith's theory by Dr. Reid, who seems to have overlooked the answer. Reid's Inquiry into the Human Mind, 8vo. p. 332.

† Vol. I. p. 46.

Such is the solution which Dr. Smith has given of this celebrated question, and such the reply which his general account of vision furnishes to one objection against it. But there are others which, in my opinion, cannot be so easily repelled. Before I offer these, however, I beg leave to remark, that although it were proved, as I think it may be, that he is mistaken in the fact of objects appearing single, when their pictures fall upon the middle or other corresponding points of the retinas, still the truth of what is peculiar to him * of the solution he gives, might remain unshaken. Objects, it may be said, are constantly seen single when we direct our eyes to them in a particular manner. Their pictures must, consequently, in every such case, fall upon the same places of the retinas; and whether these be corresponding or not, the unity of the visible appearances will be owing to the connexion, which has uniformly been observed between the sensations of

* Dr. Reid attributes to Bishop Berkeley the opinion, that objects appear single to two eyes, from an experienced connexion between particular sensations of sight, and the informations of touch. But I no where find it mentioned in the works of that author; and I even think it probable, that he purposely avoided treating of the question, as he found that the solution of it, which naturally flowed from his principles of vision, was with difficulty to be reconciled to other conclusions he had derived from the same source.

those places, and the information from feeling, that the objects which cause them are single. What I shall say, therefore, upon his opinion, will tend to show, that, admitting the fact respecting corresponding points to be true, his explanation of it ought, however, to be rejected.

For, first, it may be observed, that, if we are taught by *feeling* to see objects single, notwithstanding a sensation in each eye, the informations of the former sense ought to be uniform, or else one set of visual appearances would be associated with different reports from feeling, and no certain mark afforded us which of them we should trust. Now Dr. Smith himself is obliged to confess, that we sometimes *feel* double, " as in the dark, when a button is pressed with two opposite sides of two contiguous fingers laid across; for this reason, that those opposite sides of the fingers have never been *used* to feel one but always two things at a time*." He adds, " We have learned, therefore, by experience of both senses compared together, to make their informations consistent with each other." Here, then, we find him to allow, that

* Vol. I. p. 48. Dr. Smith, however, has, from the influence of system, I suppose, mistaken this fact; for the button is *felt* double when pressed in the manner above mentioned, though we should not be in the dark, and should even *see* it to be single.

feeling is not always the predominant, but sometimes the inferior sense; that its informations are not constant and original, but changeful and derived; positions directly contrary to those he had immediately before maintained. But, in the first instance of difference between the informations of the two senses, what rule had we for determining which was the most worthy of credit? How does a blind man correct his errors of touch? If the button be felt double, because pressed by two parts *not* accustomed to feel the same thing at the same time, there must have been a period in the life of every person, when a body pressed by any two parts would have been felt double, by three parts triple, and so on. Nor could sight have corrected those deceptions, if they can be called such; for every thing, by the same hypothesis, must then have also been seen double. How came we, therefore, both to feel and see things single? Surely not by comparing the informations of the two senses together.

But, secondly, were we to grant that the sense of touch has originally and constantly informed us that objects are single, it would not follow, that we are thence taught to *see* them also single. For, since the place which an object seems to either eye to possess, manifestly depends both upon its apparent distance and its apparent direction from that eye, if visible place be, in the language of Dr. Smith, only an *idea* of real or tangible place, visible direction must bear the same relation to tangible direction; a consequence of which is, that we can never have a more accurate knowledge of the direction, in which an object may lie from any part of our bodies, by sight than by touch. Facts, however, prove the contrary. Let any person, for instance, taking a pin in his hand, endeavour, without looking, to bring its head upon a level with either of his eyes; and there are many chances to one but he will fail in the attempt, of which sight will inform him, when he turns his eye to the object. This to me is a convincing argument, that external bodies are not seen in certain directions, because they have been previously felt in them; and, consequently, that visible place, of which visible direction is a component part, is not merely a representative of the place perceived by touch. But if the place, in which an object appears to each eye separately, does not entirely depend upon any lesson from feeling, the inference is, that when an object appears in one and the same place to both eyes together, neither is this effect to be attributed solely to the informations of that sense.

Thirdly, in whatever direction an object may appear to either eye, it certainly cannot be seen in the same place by both, except at some point common to the two directions. Dr. Smith acknowledges this, and says*, that when an object is perceived single with both eyes, it is seen at the mutual intersection of the two visual rays; the visible direction of any object coinciding, according to him, with the visual ray, or the principal ray of the pencil which flows from it to the eye. Should we then even allow, that all we know by sight of the places of bodies has been borrowed from feeling, it will still be easy to show, that the rule of vision for each eye, which he has derived from such experience, that of our seeing objects in the directions of their visual rays, is inconsistent with many of the phenomena of sight with two eyes; and, consequently, that he has left unremoved the chief difficulty of his subject, which was to explain the single appearance of objects to both eyes, from those laws, or rules of vision, which affect each of them singly. For it is a well-known fact, that if two bodies of the same shape, size, and colour, be placed, one in each optic axis, they appear but as one body, provided they be at equal distances from the eyes. Agreeably to the theory of our seeing objects

* Vol. II. Remarks, p. 86.

in the direction of their visual rays, this cannot happen, except the united body appear at the intersection of the optic axes. Dr. Smith, accordingly*, maintains that it does. Now, in the first place, I appeal to experiment for a direct proof that it does not; and, in the second, I observe, that, as the two bodies in the optic axes appear as one, whether they be situated within or beyond the concurrence of those lines, and as a right line joining the bodies, and extended both ways, appears at the same time to the sight as a right line, it follows, upon admitting the fact which I have denied, that all objects in the plane of the optic axes which are seen in one position and state of the eyes, however near to us, or however remote they may in reality be, must appear to be equally distant, or rather in a line drawn through the concourse of the optic axes, parallel to the interval between the eyes, and named by opticians the *horopter*. Again, if a right line be made to pass through any part of the plane of the optic axes, at right angles to it, the portions above and below this plane are perceived to be in the same right line with the point which is situated in it, and the whole appears perpendicular to the plane. But the point in the plane

* Vol. II. Remarks, p. 86.

is seen, by the last article or proposition, in the horopter; the whole, therefore, of the perpendicular line must be seen in a plane passing through the horopter at right angles to that of the optic axes; or in other words, in the *plane of the horopter*, in which consequently all bodies will have their visible places. But this was the very opinion of Aguilonius, to which he was probably led by a similar train of reasoning; though, as a teacher, he might choose rather to ground it immediately upon an original law of our constitution.

It is probable, however, that Dr. Smith did not perceive the conclusions which might be drawn from his doctrine of objects being seen in the directions of their visual rays, since he has no where spoken of them. At any rate, it is manifest he did not admit them, as he has mentioned the following circumstance as a fact*, to which they cannot be reconciled; that, when an object is seen double, both its apparent places are situated between its real place, and the mark at which we look. For if this were just, together with what he has elsewhere advanced, phenomena ought in many cases to be observed, very different from those which are in truth found to exist. Thus, for

* Vol. I. p. 48.

example, if a right line be any where placed in the plane of the optic axes, it follows, from what he has said in one part of his book, that those points of it, through which the axes pass, must be seen united at the mark we look at, the axes crossing each other there; and from what I have just quoted, that every other point must be seen by each eye between its real place and that mark. The appearances, therefore, of all the points, if they do not lie disjoined, but are connected together in some orderly manner, will be arranged in the forms, either of two curves, both passing through the intersection of the optic axes, or of four right lines meeting one another at that point. If the right line be placed nearer to the face than the mark we look at, the apparent lines, whether curved or straight, will approach toward us from their common point, but recede from us, if the real line be situated beyond the mark. Such are the phenomena which ought to follow upon the admission of these two parts of Dr. Smith's theory of vision with two eyes, but which are not found to exist in nature. Aguilonius was at least consistent when he maintained, that all objects are seen in the plane of the horopter; while Dr. Smith, by deserting that opinion in part, seems only to have involved himself the more deeply in error.

Having now said what, I hope, will be thought sufficient to show, that the reason given by Dr. Smith, for our seeing objects single with both eyes, is neither grounded on well-attested facts, nor adequate to the explanation of the phenomena observed, I pass to the examination of the opinion of Dr. Reid.

As this neither rests upon nor includes any new fact in vision, I need only mention, in order to give an account of it*, that its author maintains with Dr. Smith, that an object is seen in the same place with both eyes, and consequently single, when its pictures fall upon the centres of the retinas, or upon points in them, which are similarly situated with respect to the centres; but differs from him in this, that he makes the property to be original, by which any two places in those membranes exhibit only one object, while Dr. Smith derives it altogether from custom †.

In my examination of the opinion of Dr.

* Inquiry into the Human Mind, c. vi. sect. 13.

† They differ also with respect to the meaning of a term; Dr. Smith calling *corresponding points*, such as have the position just mentioned, whether they represent objects single or not; whereas Dr. Reid says, that those points correspond, whatever their position may be, which represent objects single; and he appears to me not always to attend to the double use of the same term, when he speaks of the opinions of Dr. Smith.

Smith, I took occasion to remark, that the truth of what distinguished it from all others might remain unshaken, though it were proved, that objects do not appear single, when their pictures occupy any of the corresponding points of the two retinas, since custom might have associated the perceptions of touch, with the sensations of any other parts whatsoever of those membranes. The same observation will not apply with equal justice to the opinion of Dr. Reid. On the contrary, could it be shown, that the places of the two retinas, which represent an object single when each receives its picture, are not the centres, or such others as are similarly situated, an obvious inference would be, that the single appearance of the object is not occasioned by a property in those places, bestowed upon them for this special purpose by nature; it being reasonable to expect, that such a property should be found, if any where, in those parts of the retinas which are the most like to each other. I have, therefore, reserved till now, the observations which have occurred to me upon this subject, and which, when stated, must at least, raise some doubt concerning what has been regarded as true by Dr. Smith and Dr. Reid, and by almost every other writer on vision since the time of Kepler.

Anatomists have commonly taught, that the centres of the spheres, to which the cornea, the ball of the eye, and the two portions of the crystalline belong, are all placed in the same right line, hence called the optic axis, and that this being produced both ways, passes through the centres of the cornea and retina, considered as surfaces. Opticians, on their part, observe, that an object appears single to both eyes, when the axis of each is accurately directed to it; from which they infer, that the centres of the retinas agree in suggesting but one object, though each receives its picture.—Again; since it is known by experience, that, while any object is seen single, to which the optic axes are turned, others at the same distance from the eyes likewise appear so; and since the pictures of these lateral objects fall upon points in the two retinas, equidistant from their centres, and both upon the same side, that is, both to the right or left of the centres, or both above or below them, opticians conclude, that every two places of the retinas, which are similarly situated with respect to the centres, must also agree in exhibiting but one object, though pictures are received by both.

But the whole of this reasoning is built upon a circumstance in the fabric of the eye, which has been shown by some of the most eminent anatomists not to have place. For Varolius* long ago observed, that the crystalline is not situated in the middle of the eye, but more *inwardly;* and the accurate Zinn† has more lately mentioned, that if the eye be divided into a right and left half, the centre of the crystalline will be found in the inner portion. Haller‡ confirms this fact; and Winslow's|| observation, that the centres of the pupil and iris do not coincide, but that the former is nearer to the nose than the latter, is connected with it; since both Zinn and Haller agree, that the centre of the pupil is placed in the axis of the crystalline, while that of the iris is evidently in the common axis of the cornea and globe. Now, a consequence of this position of the crystalline is, that, contrary to what I believe is universally maintained, no ray of light whatsoever can pass unbent to the retina from the atmosphere, or any other medium differing in refractive power from the aqueous humour. If, then, the line joining the centres of the cornea and globe of the eye be what is called the optic axis, and if it be true, that objects appear single when we direct

* Varolii Anatomia, 12mo. p. 16.

† De Oculo, 4to. p. 127.

‡ Elementa Physiologiæ, tom. v. p. 403.

|| Winslow's Anatomy, vol. ii. p. 379, English edition, 8vo.

both these axes to them, it must be evident, to such as are acquainted with the common rules of optics, that the pictures of those objects do not fall upon the centres of the retinas, but more internally; and, therefore, that the centres and all the other points of those membranes, which by the present system are supposed to represent objects single, do in fact exhibit them double.

It will be said here, perhaps, that the line* passing from each eye, which we turn to objects when we see them single, is not a production of the common axis of the cornea and globe, but some other, disposed in such a manner, that the pictures of those objects are received by the centres of the retinas. I answer; I readily grant the possibility of the thing, but I assert, at the same time, that we have no proof of it,

* I am of opinion, that this line, or at least the line which we turn to objects when we see them most distinctly with one eye, is not the common axis of the globe and cornea. For I find, that, when I place the flame of a candle between either of my eyes, and a plane mirror, in such a manner that it may conceal its own image in the mirror from the sight of that eye, or rather that it may be a little below this image, but in the same vertical plane with it, the image of the flame, seen by reflection from the cornea, does not appear upon the middle point of this coat, but upon that point of it which is opposite to the centre of the pupil.

which is a sufficient reason for rejecting every conclusion that depends upon its truth.

Admitting, however, that objects are represented single, when their pictures fall upon the centres of the retinas, or upon any other two points which are equally distant from the centres, and both upon the same side, it appears to me, notwithstanding, to be in violation of all analogy, to ascribe this effect, with respect to the points, at least, on the right and left sides of the centres, to any peculiar property which they possess from nature. For when anatomists find, in a new species of animals, organs similar in structure to those of others they are already acquainted with, they immediately conclude, that they are also similar in regard to their use. In animals of the same species, they believe with certainty, that the organs they see in one have the same properties, as the corresponding organs of another; and, if it be possible, they attribute with greater certainty the same properties to two organs of the like kind, which are found in the same individual. Such is the influence of the rule, that resemblance of property is implied by resemblance of structure. Now it is an universal fact, that if an animal be divided into a right and left half, the corresponding parts of those organs, which exist in pairs, are found at equal distances from the plane of partition. Thus, for instance, in respect to the eyes, the two optic nerves penetrate their outward coat at the same distance from this plane. Their muscles, blood-vessels, and every other of their component parts and appendages, are arranged in the like manner; those nearest to the dividing plane, or the innermost, in the one, being similar in structure to the innermost in the other, the outermost to the outermost, and the intermediate to the intermediate. It is surely, therefore, natural to expect, that such parts should also be similar in their properties; and we in fact find this similarity to exist, whereever it can be clearly ascertained what the properties are. Every person, for example, admits, that the internal straight muscle of the right eye performs the same office, with respect to that eye, as the other internal straight muscle does with respect to the left eye. What judgment are we then to form of the opinion of Dr. Reid, which attributes the same original properties, or rather the joint possession of one original property, to places in the retinas situated at unequal distances from the general plane of partition; which makes an *external* point in one to correspond, in use, with an *internal* point in the other, and this too by a principle implanted by nature? If such things exist, they may, at least, be said to stand opposed to a most extensive analogy.

To these arguments, *a priori*, against the opinion of Dr. Reid, I shall now add others, which are derived from a consideration of its consequences.

First; Since visible place, as was formerly observed, includes in it visible distance, it is evident that, if both eyes, by virtue of an original property, see an object in the same place, distance must also be originally perceivable by sight. Dr. Reid*, however, has himself so ably shown, that we should never have acquired, by means of our eyes, any knowledge of distance, unless they had been assisted by the sense of feeling, that I forbear to say any thing more upon this head, than that the existence of no property can be admitted, which leads to the conclusion I have stated.

Secondly; If distance be not immediately perceivable by sight, the only manner, in which an original property of the eyes can affect the visible places of bodies, is by occasioning them to appear in certain directions. Now Dr. Reid maintains†, that every external point is seen in

* Inquiry into the Human Mind, chap. vi. sect. 3 & 20.
† Ibid. chap. vi. sect. 12.

the direction of a line passing from its picture on the retina, through the centre of the eye. If, therefore, this direction be the same as that suggested by the original property so often mentioned, the latter law is merely another expression for the former, and ought to be rejected as superfluous. If it be different, and should the two laws exist together, objects seen with both eyes might sometimes appear quadruple, sometimes triple, but never single. Were they to exist successively, one when we employ one eye, the other when both, an object, though at rest, should always appear to move when viewed alternately by one and by both eyes; neither of which conclusions is agreeable to experience.

Thirdly; To show in a different way, and one perhaps more easily understood, that the opinion of Dr. Reid is not consistent with the phenomena of vision it ought to explain, I shall suppose an experiment to be made upon a person who squints. But I must premise, that it appears, from the observations of Dr. Jurin* and himself†, that all such persons have one eye of a weaker sight than the other; that when both eyes are open, the weaker is turned away from objects, which are attentively viewed; but that

* Smith's Optics, Vol. II. Remarks, p. 30.

† Inquiry, chap. vi. sect. 16.

when the strong eye is closed, the weaker is pointed to objects, exactly as the former would be in the same situation; and that it likewise perceives them in similar directions. Let now the ordinary position of the person's eyes, upon whom the experiment is made, be such, that the optic axes intersect each other about an inch or two from the face; and while the other is closed, let the flame of a candle be placed in the axis of the weak eye, which I shall call the left, at the distance of some feet from it, and on the right side of the body. The flame will consequently appear in the same direction, as if his eye had no fault, and will be seen on his right, where it is in reality situated. Both eyes retaining the same position with respect to his head and each other, let the weak eye be afterward shut, and the right opened, and let another object be placed in the axis of the latter, an opake body being at the same time so disposed, as to hide from it the candle which is in the axis of the left eye. This object in the right axis will consequently appear on the left side. Now, since the two objects, which have been thus viewed separately, are situated, one to the right, and one to the left; and since they have been also *seen* in those positions, their visible places must be two, as well as their tangible, and must be remote from each other. How then should these objects appear, if, instead of being viewed alternately, each by the eye in the axis of which it is placed, they were seen by the two together; the positions and internal states of the eyes being in both cases the same? Dr. Reid must answer; They will possess but one visible place, since their pictures fall upon the centres of the two retinas, points endowed with the original property of representing objects single. But where is this one place to be found? In the axis of the right eye, or in that of the left, or between the two? In any of these cases, or in any other that can be imagined, the law of visible direction, so much insisted upon by Dr. Reid, that objects appear in the perpendiculars to their pictures upon the retina, and in truth every other law of visible direction hitherto published, must be suspended with respect to one or both eyes; unless, indeed, the united object be referred to the intersection of the optic axes, about an inch or two from the face. This, I believe, Dr. Reid would not readily admit; but if he should, another case of squinting may be imagined, in which the optic axes recede from each other, and where the same reasoning will apply without the possibility of its force being thus eluded. It now remains for me to mention, that the experiment here stated by the way of supposition, in which the optic axes cross each other near to the face, was actually made by Dr. Reid, with this result, that the two objects appeared in different places, when seen by both eyes together; and that the other experiment, in which the optic axes are supposed to diverge, was made by myself, with a similar event. Dr. Reid, however, instead of being led, by the termination of his experiment, to impute a fault to the principle from which he had expected a different one, concluded from it, that there was something unnatural, beside the squinting, in the person's eyes, upon whom it was made; though it had been previously ascertained, that objects appeared in the ordinary manner to each of them, when separately employed.

My examination of the second class of opinions, respecting the cause of the single appearance of objects to two eyes, being finished, some person, perhaps, will now say; Granting that no error can, at first sight, be shown in your arguments against those of Dr. Smith and Dr. Reid, is it not a sufficient reason for believing them fallacious, that they prove too much? If objects appear single neither from custom, nor an original property of the eyes, have we not an effect without a cause, and must there not be something wrong in the facts or reasoning which lead to such a conclusion? The answer I make is as follows: Since visible place contains in it both visible distance and visible direction, it is not necessary that the single appearance of an object, to both eyes, should depend altogether either upon custom, or an original principle of our constitution; for its visible distance to each eye may be learned from feeling, and its visible direction be given by nature; in which case, the unity of its place to the two eyes, will be owing to neither of those causes singly, but to a combination of both; and this I regard as a sufficient reply.

PART II.

Of a new Theory respecting Visible Direction, and of a Solution hence derived of the Question, why Objects are seen single with two Eyes.

I NOW proceed to offer a new opinion, why objects are seen single with two eyes; or in other words, why they appear in the same place to both, this being the light in which I view the fact to be explained.

In every part of natural philosophy, accidents often lead to discoveries, which reason alone might not easily have reached. Under this cover I hope to shelter myself from the charge of presumption, in venturing to give the solution of a problem, upon which the talents of many persons of great learning and genius have been unsuccessfully employed; for should I prove more fortunate than such men have been, this must be attributed to the knowledge of a circumstance I observed by chance, in repeating some very common experiments.

The visible place of an object being composed, as I have already several times remarked, of its visible distance and visible direction, to show how it may appear the same to both eyes, it will be necessary to explain, in what manner the distance and direction, which are perceived by one eye, may coincide with those which are perceived by the other: and first with respect to the distance.

In judging of distance by sight, we frequently make considerable mistakes, even when the objects are not very remote; but no person, I believe, has ever observed, that while an object seemed to one of his eyes at a certain distance, it has appeared to the other to be at a different distance, and from this circumstance alone has been seen double; or, to express the same thing in another way, that while the visible appearance of an object to one eye, covered the visible appearance of the same object to the other eye, the two appearances did not seem entirely to coincide, and make one, but were seen separate by the two eyes. I do not stop to give the reason of this fact, which must be plain to those who are acquainted with Bishop Berkeley's theory of visible distance; but proceed to mention, that the difficulty in finding a true and sufficient cause for the union of the two visible places of one or two objects to two eyes, must therefore consist altogether in showing, in what manner the two apparent directions may coincide, consistently with the attending phenomena.

Since Kepler's great discovery of the seat and manner of vision, there have been, as far as I know, only two theories offered respecting the apparent directions of objects. One is, that they are perceived in the direction of lines passing from their pictures on the retina, through the centre of the eye; the other, that their apparent directions coincide with their visual rays*. But both of these theories are inconsistent with the phenomena of single vision with two eyes. For according to neither of them can an object, placed at the concourse of the optic axes, be seen single, unless we have a most accurate knowledge of its distance; nor will either admit two objects to be seen as one, which are situated in the optic axes, whether on this side, or beyond where they meet, unless the united object be referred by sight to their very point of intersection; both of which conclusions are contradicted by experience. It is evident, therefore, that some other theory of visible direction is required, which shall not be liable to

* Mr. D'Alembert has said (Opuscules Mathematiques, Tom. I. p. 265) that all optical writers before him had regarded it as an axiom, that every visual point is seen in the direction of its visual ray. But the assertion is not well founded. For Kepler long ago taught (Paralipomena in Vitellionem, p. 173), that objects are perceived in lines passing from their pictures upon the retina, through the centre of the eye; in which he was followed by Dechales and Doctor Porterfield; to the latter of whom Dr. Reid improperly attributes the discovery of the same supposed law.

these objections; and such a theory, I hope, I shall bring forward in the following propositions, after mentioning the meanings which I affix to several terms I shall frequently employ.

EXPLANATION OF TERMS.

I. When a small object is so placed with respect to either eye, as to be seen more distinctly than in any other situation, I say it is then in the *optic axis*, or the axis of that eye; and if another small body be interposed between the former and the eye, so as to conceal it, and if a line joining the two be produced till it falls upon the cornea, I call this line the *optic axis*, or the axis of the eye; leaving for future determination the precise point of the cornea it falls upon, or what part of the retina receives the picture of an object which is placed in it.

II. When the two optic axes are directed to a small object not very distant, they may be conceived to form two sides of a triangle, the base of which is the interval between the points of the corneas, where the axes enter the eyes; but if the object be very distant, then they may be supposed to be two sides of a parallelogram, whose base is the same interval. To avoid circumlocution, I shall call this interval the *visual base*.

III. If there be drawn a line from the middle of the visual base, through the point of intersection of the optic axes, or parallel to them, if they be parallel to each other, I name it the *common axis**. This term, I believe, was invented by Alhazen; but with him it signified a line drawn from the centre of the junction of the optic nerves, through the middle of the interval between the centres of the retinas. Such a line was consequently immoveable. As the term, however, is not in modern use, no mistake can arise from confounding the two meanings, and the reason will soon be seen, why I employ it in the sense I have mentioned. Those who are acquainted with the writings of the older opticians, will perceive, that I give it nearly the same signification as they did to their *common radius*.

* It may be said, perhaps, that as I do not define the points of the corneas, upon which the optic axes fall, I cannot, with propriety, desire the line which connects them to be divided. To this I answer, that it is not necessary for the purpose I have mentioned, that they should be defined; if it be granted to me, and I think it cannot be refused, that upon whatever point of the right cornea the right axis falls, the left axis will fall upon a similarly situated point of the left cornea; that is, if this point of the right cornea be at any given distance from its middle, and upon the inside of it, the corresponding point of the left cornea will be at the same distance from the middle of this, and also upon its inside. Whatever extent, therefore, the line connecting these places of the corneas may have, its middle point will be the same.

PROPOSITION I.

Objects situated in the Optic Axis, do not appear to be in that Line, but in the Common Axis.

Every person knows, that, if an object be viewed through two small holes, one applied to each eye, the two holes appear but as one. The theories hitherto invented afford two explanations of this fact. According to Aguilonius, Dechales, Dr. Porterfield and Dr. Smith, the two holes, or rather their borders, will be seen in the same place as the object viewed through them, and will consequently appear united, for the same reason, that the object itself is seen single. But whoever makes the experiment will distinctly perceive, that the united hole is much nearer to him than the object; not to mention, that any fallacy on this head might be corrected by the information from the sense of touch, that the card, or other substance, in which the holes have been made, is within an inch or less of our face. The other explanation is that furnished by the theory of Dr. Reid. According to it, the centres of the retinas, which in this experiment receive the pictures of the holes, will, by an original property, represent but one. This theory, however, though it makes the two holes to appear one, does not determine where this one is to be seen. It cannot be seen in only one of the perpendiculars to the images upon the retinas, for no reason can be given why this law of visible direction, which Dr. Reid thinks established beyond dispute, if it operates at all, should not operate upon both eyes at the same time; and if it be seen by both eyes in such lines, it must appear where those lines cross each other, that is, in the same place with the object viewed through the holes, which, as I have already mentioned, is contrary to experience. Nor is it seen in any direction, the consequence of a law affecting both eyes considered as one organ, but suspended when each eye is used separately. For when the two holes appear one, if we pay attention to its situation, and then close one eye, the truly single hole will be seen by the eye remaining open, in exactly the same direction as the apparently single hole was by both eyes.

Hitherto I have supposed the holes almost touching the face. But they have the same unity of appearance, in whatever parts of the optic axes they are placed; whether both be at the same distance from the eyes, or one be close to the eye in the axis of which it is, and the other almost contiguous to the object seen through them. If a line, therefore, be drawn from the object to one of the eyes, it will represent all the real or tangible positions of the hole, which allow the object to be seen by that eye, and the whole of it will coincide with the optic axis. Let a similar line be drawn to the other eye, and the two must appear but as one line; for if they do not, the two holes in the optic axes will not, at every distance, appear one, whereas experiments prove that they do. This united line will, therefore, represent the visible direction of every object situated in either of the optic axes. But the end of it, which is toward the face, is seen by the right eye to the left, and by the left eye as much to the right. It must be seen then in the middle between the two, and, consequently, in the *common axis.* And as its other extremity coincides with the point where the optic axes intersect each other, the whole of it must lie in the common axis. Hence the truth of the proposition is evident, that *objects, situated in the optic axis, do not appear to be in that line, but in the common axis.*

Many other experiments might be mentioned which demonstrate the same thing. If, for example, the head of a pin, or of a needle, be interposed between each eye, and any small object to which both the optic axes are directed, the heads of the two pins or needles will constantly appear as one in the common axis. When the heads, however, are near to the eyes, this experiment is not so satisfactory as the former, since, in these positions, they seem as broad transparent shadows, for reasons known to every person a little conversant in optics; whereas the holes appear well defined, though almost touching us. Again; if we hold two thin rulers in such a manner, that their sharp edges shall be in the optic axes, one in each, or rather a little below them, the two edges will be seen united in the common axis, and this apparent edge will seem of the same length with that of either of the real edges, when seen alone by the eye in the axis of which it is placed. If instead of two rulers we employ two strings of different colours, as red and green, the like unity of appearance will be observed. But in this experiment it frequently happens, that, contrary to what we might naturally expect, only one of the strings is seen at a time. When, however, only one is seen, its apparent situation is exactly the same as that of the string, compounded, if I may so express myself, of the two when seen together; and hence we have a convincing proof, if any were wanted, that the single appearances of objects must depend upon some law of visible direction affecting each eye, when employed by itself, in the same manner as when it is used conjointly with the other*.

* Du Tour expected, that if two objects of different colours were seen in the same place by both eyes, which however he says, he was never able to observe, the colour of the apparently united object would be compounded of those of the two really single objects. Memoires des Savans Etrangers, tom. iv. p. 500. And Dr. Reid mentions expressly that it is so compounded. Inquiry, p. 293. But in all my experiments upon this subject I have remarked, that, when the two objects appeared united, each was seen, notwithstanding, in its proper colour; the red, for example, appearing as it were through a transparent green, and the green, in the same experiment, as through a transparent red. Nor is there any thing in this inconsistent with the received doctrine of the composition of colours. For in every instance of the production of a new colour, from rays of different colours being at the same time sent to the eye, these rays fall upon the same sentient extremities of the same nerve. But, in the case before us, the differently-coloured rays fall upon the sentient extremities of two different nerves, which have no communication with each other, except through the medium of the brain. We have greater reason, therefore, for expecting, that the colours impressed upon the two eyes, should be perceived uncompounded, than there is for two colours being perceived separately, which are impressed upon two different parts of the same eye.

From the fact of the two colours being thus perceived distinct from each other, I would infer, by analogy, a mode of argument indeed often fallacious, that if it were possible for us to hear any one sound with one ear only, and another sound with the other ear only, such sounds would in no case

PROPOSITION II.

Objects, situated in the Common Axis, do not appear to be in that Line, but in the Axis of the Eye, by which they are not seen.

The facts which demonstrate the truth of this proposition, are both numerous and common. If a piece of wire, or any other substance, representing a physical line, be placed in the common axis, with one of its extremities near to the visual base, and if both the optic axes be directed to its farther or distant extremity, instead of one, two wires will be seen, meeting each other at their farther ends, and gradually diverging as they approach the face, till they apparently terminate at the eyes. If the right eye be closed, the wire which seemed

coalesce either wholly or in part, as two sounds frequently do, when heard at the same time by one ear; that consequently, if the sounds of one musical instrument were to be heard by one ear only, and those of another, by the other ear only, we could have little or no perception of harmony from such sounds; and that, if in any succession of sounds emitted by one instrument, we were to hear the 1st, 3d, 5th, and so on, by one ear only, and the 2d, 4th, 6th, and so on, by the other ear only, we should be deprived, in a considerable degree, of the melody of such sounds, as this seems to depend in a great measure upon a new impression being made upon the auditory nerve by one sound, before the impression of the sound immediately preceding has passed away.

to terminate at the left eye, disappears; and if the left eye be closed, then the other wire disappears, whose termination was at the right eye. The real wire, therefore, in the common axis, appears to the right eye to be situated in the axis of the left, and to the left eye to be situated in the axis of the right, agreeably to what the proposition asserts.

The following experiments will illustrate and confirm both this and the preceding proposition. Through a piece of card, or pasteboard, let two small holes be made, the interval between which is such, that while a very remote object is seen through one of them by the right eye, the same object may be seen through the other by the left eye. Make afterward another hole in the card, or pasteboard, exactly in the middle between the two former; and let the object be viewed through them as before. These, or the outer holes, will now appear one, precisely where the sense of feeling indicates the middle hole to be; while the middle hole will appear as two, which seemingly occupy the places of the real outer ones. The two appearances of the middle hole, which is placed by construction in the common axis, are therefore seen in the optic axes; and as the left is not seen when the right eye is shut, nor the right when the left eye is shut, each appearance is observed in the axis of the eye, by which it is not seen. As I have supposed the distance between the outer holes to be adapted to the interval of the eyes when they are directed to a very remote object, the optic axes may, in this case, be regarded as parallel to each other. The object, therefore, will still be seen through those holes, though the distance of the card from the eyes be considerably varied; and at all the different distances, the same appearances will be observed, as those which have been mentioned.

Again; take three strings of different colours, as red, yellow, and green, and fasten, by means of a pin, one end of each to the same point of a table. Place now their loose ends in such a manner, that when you look at the pin with both eyes, the visual base being parallel to the edge of the table, the red string may lie in the axis of the right eye, the green in that of the left, and the yellow in the common axis. When things are thus disposed, and both eyes are directed to the pin, the red and green strings, instead of appearing separate, each in one of the optic axes, and inclined to the visual base or edge of the table, will now be seen occupying but one place, either together or successively, as was formerly mentioned, and at right angles to the visual base, or edge of the table; in short, exactly in the situation, which the yellow string in reality possesses; and the yellow string, instead of appearing single in the common axis, and perpendicular to the visual base, will now be seen as two, each inclined to the base; that seen by the right eye, apparently occupying the place in reality possessed by the green string, and that seen by the left eye, the place of the red string.

PROPOSITION III.

Objects, situated in any Line drawn through the mutual Intersection of the Optic Axes to the Visual Base, do not appear to be in that Line, but in another, drawn through the same Intersection, to a Point in the Visual Base distant half this Base from the similar Extremity of the former Line, towards the left, if the Objects be seen by the Right Eye, but towards the right, if seen by the Left Eye.

Two cases of this proposition have already been proved. For it has been shown by the first proposition, that objects, placed in the axis of either eye, appear to it to be situated in the common axis. But the common axis is a line drawn through the mutual intersection of the optic axes to the visual base, and its termination there is distant, by construction, half that base, from the similar terminations of the axes of both eyes, to the left of the right axis, and to the right of the left. Again, it has been shown by the second proposition, that objects, placed in the common axis, appear to each eye to be situated in the axis of the other; and the terminations of both optic axes, at the visual base, are distant half this base, from the similar termination of the common axis, the left being to its right, and the right to its left.

Let it now be supposed that two objects, one placed in the axis of either eye, the right for instance, and the other in the common axis, be viewed at the same time by that eye, it is evident that the visible directions of both will be equally removed to the left, from their real positions. But such an alteration of visible direction, from real position, cannot be imagined to happen, with respect to objects placed in the optic and common axes, unless a similar effect be, at the same time, produced upon such as are situated any where between those lines, or in their vicinity. Facts confirm this: If a line, for example, be drawn through the intersection of the optic axes to a point in the visual base, exactly in the middle between the terminations there of the right and common axes, its apparent situation, to the right eye, will be found to have the same relation to the apparent situations of lines placed in the right and common axes, as its real situation has to the real situations of such lines. And the like will be found, by observation, to be true of every other line, which may be drawn through the point of intersection of the optic axes to the visual base.

The whole of what has here been said may be illustrated and confirmed, by having again recourse to the experiments with strings of different colours. In formerly describing those experiments, I did not mention all the appearances which occurred upon making them, but only such, as had immediate reference to the points then under consideration. When, for instance, a red string was placed in the axis of the right eye, and a green one in that of the left, I said that they both appeared in the common axis. But this is not the only phenomenon to be observed with respect to their apparent number in this experiment. For as the red string is also seen by the left eye, and the green by the right, two other strings become visible, beside that in the common axis, the apparent positions of both of which will be found to be the same with those, which ought to follow from the present proposition. Should now a yellow string be placed between the two former, as in the proof of the second proposition, its appearance to the right eye will bisect the space between the appearances of the red and green strings to that eye; and the like will be true with respect to the appearances of the three strings to the left eye, agreeably to what the same proposition teaches us to expect.

I believe I need scarcely remark, that, although in most of the proofs and illustrations of these propositions, I have confined myself to the visible appearances of lines *between* the intersection of the optic axes and the visual base, the same things, however, must be equally true of those lines, when they are produced beyond the intersection, with this difference only, that, while the portions within, seem, to the right eye, to be farther situated to the left than they really are, but to the left eye farther to the right, the portions beyond the intersection will seem to the right eye to the right of their real positions, but to the left eye to the left of them. For it is manifest, that, if a line be seen by one eye in a certain direction, a prolongation of it must be seen in the same direction; and that, if a line be made to turn upon any point in itself, the two extremities must move contrary ways.

Should the optic axes be parallel to each other, the same proofs and illustrations will still apply, since we may here suppose them to meet at an infinite distance from the visual base. In this case, the visible appearances of lines, drawn from this supposed point of intersection to the visual base, will be parallel to the real lines, and distant half this base from them, through their whole extent.

As I have thus, I think, sufficiently proved, that the apparent directions of objects are governed by a law, different from any which has hitherto been thought to exist, I shall now proceed to state, in a few words, in what manner the phenomena of single and double vision with two eyes are dependant upon it.

I formerly mentioned, that, since an object is never seen double, merely from its being seen at different distances by the two eyes, the only difficulty in explaining its single appearance consists in showing how its two visible directions may coincide, consistently with the attending phenomena. But we are enabled to do this, with the utmost ease, by the theory I have endeavoured to establish. For, if the question be concerning an object at the concourse of the optic axes, I say it is seen single, because its two similar appearances, in regard to size, shape, and colour, are seen by both eyes in one and the same direction, or, if you will, in two directions, which coincide with each other through the whole of their extent. It therefore matters not, whether the distance be truly or falsely estimated; whether the object be thought to touch our eyes, or to be infinitely remote. And hence we have a reason, which no other theory of visible direction affords, why objects appeared single to the young gentleman mentioned by Mr. Cheselden, immediately after his being couched, and before he could have learned to judge of distance by sight.

When two similar objects are placed in the optic axes, one in each, at equal distances from the eyes, they will appear in the same place, and therefore one, for the same reason that a truly single object, in the concourse of the optic axes, is seen single. Here again, as the two visible directions coincide in every point, it is not necessary that the united appearance should be judged to be at any particular distance; that it should be referred, for instance, to the concourse of the optic axes, where the two other theories of visible direction are obliged to place it, in opposition to the plainest observations.

Objects, any where in the horopter, will be seen single, because their apparent directions to the two eyes will then completely coincide. And for a contrary reason, those placed in any other part of the plane of the optic axes will appear double. To make these things evident, let a line pass through the point of intersection of the optic axes and any given object, to the visual base, which is to be produced, if necessary; and let it be called the line of the object's real position. Take afterward, in the visual base, or its production, two points, one on each side of the line of real position, and both distant from its termination there, half the visual base. Lines drawn from these points, through the point of intersection of the optic axes, must consequently contain the two visible positions of the object. But when this is situated in the horopter, the line of real position will coincide with the horopter, and will not therefore reach the visual base, unless at an infinite distance from the eyes. For which reason, the two lines, containing the visible positions of the object, must fall upon the visual base at a like distance, and must consequently be regarded as coinciding with each other. When the object is not in the horopter, the two lines of visible direction will be found, by the same means, not to coincide.

That I might simplify a matter, which under my management, must, I fear, still be of difficult apprehension, I have, in expressing the law of vision, so frequently mentioned, purposely confined it to objects situated in the plane of the optic axes. But in persons who do not squint, or whose eyes are not distorted by external violence, the two appearances of an object, seen double, are always, either in that plane, or in some one parallel to it; so that, if the visual base be parallel to the horizon, a line joining the two appearances will, in every case, be also parallel to the horizon. Whoever then is able to explain, why objects in the plane of the optic axes appear either single or double, may readily give a reason for the like appearances of such as are placed any where else. Not to spend much time, therefore, upon this part of the subject, I shall shortly observe, that if planes be supposed to pass through the two optic and common axes, perpendicular to that in which they all lie, and if two lines be drawn from any point of the common intersection of the former planes to the visual base, one along each of the perpendicular planes which pass through the optic axes, these two lines will appear as one, in the perpendicular plane of the common axis; the single visible line, however, possessing the same elevation, in regard to the horizon, as the two real lines: And again, that, if a line be drawn from any point of the same intersection to the visual base, along the perpendicular plane of the common axis, it will appear as two, one in each of the planes which pass through the optic axes; the two visible lines having the same inclination to the horizon in their progress to the visual base, as the real single one. In this manner, every thing may be shown to be true, with respect to the single and double appearances of objects without the plane of the optic axes, which has already been done with regard to those placed in it. But farther; since any point, taken at pleasure, in the common intersection of the three perpendicular planes, appears single, the whole of the line of intersection must appear so, and likewise every point of a plane made to pass through it, parallel to the visual base. Such a plane necessarily includes the horopter, and is the same as that, which is called by Aguilonius the plane of the horopter.

To exemplify the principal property of this plane, I shall mention an experiment, which at first I did not understand, though the result was a direct consequence of my own principles. I suspended a fine chord at right angles to the horizon, and retreating a step or two, I looked steadily at a point in it, which was upon a level with my eyes. The chord, in these circumstances, appeared single; but whenever I directed my eyes to any other point of it, either above or below the former, two chords would appear, crossing each other at the part, to which the eyes were directed. In the first case, the whole chord was in the plane of the horopter, but in every other, only that point of it to which both eyes happened to be turned. A conclusion from this experiment is, that no object, which is truly perpendicular to the horizon, will appear to be so, while our bodies are erect, unless we direct our eyes to a point in it exactly upon a level with themselves.

It was once my intention to subjoin here several instances, from the most approved authors, of inaccurate descriptions of the single and double appearances of objects; in order to show, that the theory of visible direction, which I have advanced, is not only consistent with the universally received facts, but that it also discovers to us, some minute errors, which unguided sense has committed upon this subject; it being, perhaps, one of the surest tests of the soundness, as well as one of the chief uses, of theories in philosophy, that they lead to the knowledge of what, otherwise, might have remained for ever hidden. But fearing I have already proved tiresome, I give up this design, and hasten to the consideration of some consequences from my theory, which seem to me both curious and important, and which, when first mentioned, may appear to carry with them their own refutation.

PART III.

Of some Consequences from the foregoing Theory of Objects being seen single with two Eyes, together with the Explanation of several other Phenomena of Vision.

IT has hitherto, I believe, been thought by opticians, that, if the position of the eye be unchanged, the visible direction of an object will be the same, as long as its picture occupies any one point of the retina; and that, in every different position of the eye, a picture, which continues to occupy the same point of the retina, will represent its object in a different direction. But if the theory be just, which I have advanced in the preceding part of this Essay, neither of those opinions can be universally true. For it follows, from what was there mentioned, that if one of the optic axes be kept fixed, and the other be at different times variously bent toward it, objects, though situated in the fixed axis, will nevertheless change their visible directions, with every variation of the moveable axis; since they must always appear in the common axis, which alters its position with every change of the moveable axis: And again, that, if the two optic axes should vary their inclinations to each other in such a manner, that the common axis, may, notwithstanding, remain fixed, an object placed in either optic axis, and following it in every motion, will possess but one visible direction, in all this variety of real positions. That these conclusions from my theory, or rather parts of it, are true in fact, I can assert upon the authority of observations, and I shall now attempt to trace them both to a common principle, by means of some experiments, which were instituted with a very different view.

When we have looked steadily for some time at the flame of a candle, or any other luminous body, a coloured spot will appear upon every object, to which we shortly after direct our eyes, accompanying them in all their motions, and exactly covering the point, which we desire to see the most accurately. Whatever therefore can be proved concerning the apparent direction of such a spot, in any given position of the eyes, must likewise be true in the same position of the eyes, with regard to the apparent direction of an object, situated at the concurrence of the optic axes; as its pictures must occupy, in this case, the very parts of the retinas, upon the affections of which the illusion of the spot depends. This being premised, I shall now relate one or two observations, respecting the apparent directions of the spot, and consequently upon those of external objects, which, as far as I know, have not been mentioned by any other person.

1. The spot is always seen single, whether the surface, upon which it is projected, be touching the face, or at the greatest distance from us; and the reason is plain. For the parts of the retinas, by whose affections from the luminous body it is occasioned, are those likewise which receive the pictures of objects, placed at the intersection of the optic axes; and as such objects always appear single, so must also the spot. The fact indeed is so open to observation, and its cause so easily shown, that I should scarcely have thought of mentioning it, had not Dr. Darwin* lately told us, that the spot is seen double, as often as the eyes are directed to an object more or less distant than the luminous body which gave rise to it. With respect to our different assertions upon this point, I shall only say, that I have made the experiment, I believe, upward of an hundred times, uniformly with the same result; and that, if the spot ever appears double, this must be from some cause very wide of a change

* Philosoph. Transact. for 1786, p. 318. Dr. Darwin indeed, says, p. 341, that Buffon had observed the same fact; but it is evident he has mistaken that author's meaning.

in the mutual inclination of the optic axes, to which he attributes it*.

2. The spot not only appears single in every ordinary position of the optic axes, but cannot even be made to appear double, by any means whatsoever. If it be projected, for example, upon a piece of white paper, whoever makes the trial will find, that, although, on pressing one eye upward or downward, or to either side, the paper will be seen double, yet the spot will always appear single, and to possess its former place on the paper, as seen by the eye, which is not disturbed. Before I knew the result of this experiment, I had imagined, that, the position of one eye being forcibly altered, the

* The only way, in which I think it possible for the spot to appear double, consistently with the universally acknowledged fact, that an object at the intersection of the optic axes is always seen single, is this, that, when the intersection is near to the face, an object placed in it shall not send its pictures to the same points of the two retinas, as it does, when the intersection is more remote. And such I once hoped to find to be the case; for I had formed, upon the supposition of its truth, a more plausible account of the manner in which the eyes are fitted to receive, successively, pictures equally distinct from objects at different distances, than any I had met with. But, after many experiments to ascertain the matter, I was obliged to return to the common opinion, that the picture of an object in the optic axis, whatever be its distance from the eye, is always received upon the same point of the retina.

external situation of the spot, which was suggested by the affection of that eye, would likewise be altered, and the spot by consequence be seen double. As the event, however, was contrary to my expectation, I began to suspect some cause of fallacy had been overlooked, which at length I thought might be this, that the spot had been seen by that eye only whose position was not disturbed, the violence, suffered by the other, interrupting the due exercise of its functions. To determine, therefore, whether my conjecture was well founded or not, I made another experiment, which is mentioned in the following article:

3. Having looked steadily for some time at the flame of a candle, with *one* eye only, I directed afterward, with both eyes open, my attention to the middle of a sheet of paper, a few feet distant; the consequence of which was, that a spot appeared upon it in the same manner, as if I had viewed the flame with both eyes, though somewhat fainter. My attention remaining fixed upon the sheet, I now pushed the eye, by which the spot was seen, successively upward and downward, to the right and to the left, and in every oblique direction; the spot however never altered its position, but kept constantly upon the middle of the appearance of the paper, perceived by the undistorted

eye, though the appearance of the paper to the distorted eye, was always separate from the former, and the sheet consequently seen double. My conjecture, therefore, was proved to be ill grounded, and all suspicion of fallacy in the former experiment ceased.

Now it is evident, from these two last experiments, that the situation of the spot does not depend upon the bare position of the eyes, or else, in the former of them, it would have appeared double, and in the latter, it would have been moved from the middle of the paper, when the only eye by which it was seen was pushed from its place. Neither can it depend upon the bare position of the muscles of the eye, as these were also moved in the same experiments; nor upon any affection whatever of the optic nerve. For since this last substance is altogether passive, even in those motions of the eyes which do occasion a change of the spot's situation, every alteration, induced upon the nerve by those motions, must be ultimately ascribed to a change of its position; and we have seen, that similar changes of its position have been produced by external violence, without any alteration of the spot's situation. The apparent situation of the spot being, therefore, dependant upon none of these circumstances, and being at the same time affected by the *voluntary* motions of the eye, it must, I think, be necessarily owing to the *action* of the muscles, by which these motions are performed. Assuming then as true, that the apparent direction of an object, which sends its picture to any given point of the retina, depends upon the state of action existing at the same time in the muscles of the eye, and consequently that it cannot be altered, except by a change in the state of that action, I shall proceed to trace to this principle, several phenomena of vision, particularly the uniform singleness of the spot already described, and the two facts respecting the visible directions of objects in the optic axis, which were mentioned in the beginning of this part of my Essay.

The thing itself is universally acknowledged, though a dispute has arisen whether custom or an original property be the cause, that every voluntary motion of one eye, in persons who do not squint, is attended with a corresponding motion in the other. Now as all voluntary motions are produced by muscular action, it follows, that every state of action, in the muscles of one eye, has its corresponding state in those of the other, and that the two are constantly conjoined. When, therefore, the spot appears single to both eyes in their free positions, the states of action in the muscles must be such, that the direction, in which it is seen by one eye, coincides with that in which it is seen by the other. But, if we push one eye from its place, no change is hereby made in the action of its muscles; for the state of action in those of the free eye is confessedly the same as it was; and it will be attended with a corresponding state in those of the distorted eye; in proof of which it may be observed, that, whenever the pressure is removed, the distorted eye immediately returns to its former position, without the aid of any new muscular effort. The conclusion then is, that, since there has been no alteration in the action of its muscles, neither ought there to be any in the direction of the spot seen by it, which is the fact to be explained.

Hence also is to be derived the true reason, why objects appear double, when one eye is pushed from its place. For as their pictures must fall upon points of the retina in this eye, different from what they formerly possessed; and as no change is made, by the distortion, upon the visible direction, suggested by any part of the retina, the objects will be seen by the pressed eye, exactly in the same directions as they would have been, *before* it was pressed, had the pictures *then* fallen upon the points of the retina, which they *now* occupy. They must therefore be *now* seen in different directions by the two eyes, and consequently double. An experiment with a contrary event will confirm this explanation, and likewise show more clearly, in what I differ from those who have endeavoured to account for the same fact. Both eyes being open, let one of them be pushed from its situation, and let two similar objects, such as two pieces of money of the same metal and stamp, be afterward so placed, that one shall lie in each optic axis; these two objects will now appear to be one, and the object so compounded will be seen in the place, to which the undisturbed eye refers the truly single object lying in its axis.

Another inference from this doctrine is, that, if the eyes are in any very unusual position with respect to each other from the action of their own muscles, as in persons who squint, two objects placed in the optic axes, one in each, will not appear as one object; for each will be seen in the direction, which is determined by the state of action in the muscles of the eye, upon whose retina its picture falls; and as this state, in one eye, does not correspond with that in the other, the directions cannot coincide. This conclusion is verified by the result of an experiment of Dr. Reid upon a person, affected with strabismus, and by that of another, made by myself, both of which have been already related.

To explain, therefore, why an object in the optic axis appears at different times in different directions, though the axis be kept fixed, it is only necessary to show, that, whenever this happens, a change, notwithstanding, occurs in the actions of the muscles which move the eye. With this view, I observe, that the motions of that organ may be divided into two sets; the first, consisting of those, by which one eye is carried along with the other, upward and downward, to the right and to the left, and in every oblique direction, the interval between the pupils remaining constantly the same; the second, of the motions of the pupils, or the anterior parts of the eyes, to and from each other. Supposing now, that both the optic axes are perpendicular to the visual base; should the left axis be afterward inclined to the right side, the natural tendency of the right axis is to incline equally to the same side, so as to preserve its former parallelism to the left. This tendency, however, in the right axis to follow the left, may be counteracted by an effort of the muscles, which regulate the interval of the pupils, until the two axes intersect each other within two or three inches of the face. But it is evident, that the same degree of muscular force will be required to retain the right eye in its original position, as is necessary to give to the left eye its motion toward the right; and hence, that, in every different inclination of the left axis to the right, an object placed in the latter, though its real position be unchanged, will, nevertheless, appear in a different direction, in consequence of the different state of action in the muscles of the right eye, which accompanies every new degree of inclination of the axes to each other. As the object must always appear in the common axis, the alteration, in this example, of its visible direction, from an *increase* of the mutual inclinations of the optic axes, will be from left to right; but when the inclination decreases, from right to left. If the right axis be the one which is moved, and the left fixed, the alterations of visible direction in an object placed in the latter, from similar changes in their inclinations, will be contrary to those which have just been mentioned.

The reason also can now be made to appear, why an object, preserving constantly its place in the optic axis, may, in a considerable variety of its real positions, possess but one visible direction. For, in such cases, the change of its visible direction, which might be expected to accompany the motion of the eye in the axis of which it is situated, is prevented from occurring, by a tendency to a change of its visible direction the contrary way, produced by the muscular actions which regulate the mutual distance of the pupils. To know how this happens, suppose the two optic axes to be parallel to each other, and perpendicular to the visual base; and let a physical line be placed in either of them, so as entirely to coincide with it. This line will, therefore, not only be in reality perpendicular to the visual base, but will, in the present state of things, likewise appear so.—Incline afterward both the axes equally to the left side, and it is manifest that the line coinciding, say, with the right axis, must appear equally inclined. Let now the right axis be kept fixed, and the left be carried back again, and its motion continued, until it be as much inclined toward the right side, as itself was just before, and as the right axis is still to the left side; the consequence will be, that the line in the right axis must again be seen perpendicular to the visual base; for such is the present position of the common axis. Here then we have had two opposite causes of change of apparent direction acting in succession. The muscular actions, producing the joint motions of the eyes, first bent the visible position of a line, in the right optic axis, from a perpendicular to the visual base toward the left; and the muscular actions, which regulate the mutual distances of the pupils, by *increasing* the inclinations of the axes to each other, moved it afterward, from the left to the right, back again to a perpendicular to the visual base. Let these two causes act together, and it is plain, that no observable effect will be produced by either, as long as they are thus proportioned. When they are not so, only the difference of their forces will be exhibited by the phenomena.

But farther; to show the extent of this theory of visible direction being dependant upon the actions of the muscles of the eyes, I shall now apply it to the explanation of an instance of apparent motion, which at first may be thought to furnish an argument against it. Look with one eye, the other being closed, at any remote object through a small hole in a card. If you should afterward suddenly attempt to view the hole itself accurately, with the same eye, you will observe both it and the distant object, particularly the latter, to move from left to right, if the right eye be used; but if the left eye be the one employed, then from right to left. Shift now your attention as suddenly back from the hole to the object seen through it, and both will return to the places they formerly occupied. In this experiment, no real change can be supposed to have occurred in the position of the distant object; and had any happened with respect to either the eye or the hole, the object would not have been seen through the latter. No other fallacy, therefore, exists here, than that things, which are truly at rest, appear, notwithstanding, to be in motion.

The argument, which I have mentioned may hence be derived against my theory, is this: The visible directions of objects, in the optic axis which remained fixed, were formerly said to be altered, because a new state of muscular exertion was required to keep it so, in every different degree of the inclination to it of the moveable axis. But in the last experiment, there seems no good reason for supposing any change in the inclination of the moveable axis to the other; for, as one eye is closed, the obvious intention of directing the two axes to the same object, which is, that we may see it single, no longer exists. If then an apparent lateral motion be, in one instance, observed in objects truly at rest, without any change of the interval of the pupils, may not every other motion of the like kind be also independent of the muscular actions, which regulate that interval?

It is evident, that this argument rests altogether upon the supposition, that in the experiment just mentioned, no alteration occurs in the interval of the pupils. Now, we may be easily convinced, that some alteration does occur, by applying a finger to the closed eye, which will, by this means, be felt to move toward the nose, when we endeavour to view the hole accurately, and from the nose, when we carry our attention back again to the remote object. Were, indeed, the opinion of Aguilonius * just, that the mind perceives only those objects distinctly, which are situated at the concourse of the optic axes, whether they are seen with one or with two eyes, both the necessity and the degree of the alteration would be clearly ascertained. But as this opinion is not just, which I mean to prove from experiments in a succeeding part of this work, I shall proceed to give another reason, and I think the true one, why the interval of the pupils should be as much altered, when we look with one eye at objects successively, which are placed at different distances, as if we were to view them with both.

It is a fact, for which I have the authority of experiments almost without number, though I do not recollect to have seen it mentioned by any author beside Dr. Porterfield, that every

* Optica, p. 84.

change of the mutual positions of the optic axes is conjoined, in persons who do not squint, with a change of the power, in both eyes, to refract the rays of light which fall upon them. When the axes are parallel to each other, the eyes are in their lowest refracting state; but in their highest, when the axes are mutually intersected within two or three inches of the face; every intermediate inclination being also conjoined with an intermediate degree of refracting power. Now, since those objects are seen most distinctly, the radious pencils from which are accurately brought to points in the retina, it follows, that, although we employ one eye only, the same reason exists for adjusting its refractive power to their distances, as if we saw with both. When, therefore, we view a remote object with one eye, we use it in its lowest refracting state, which, I have observed, is conjoined with the widest interval of the pupils. Should we afterward attempt to see accurately a very near object, the eye will assume its highest refractive state, and the interval of the pupils be lessened; the consequence of which must be, that both the objects lying in the optic axis will appear to move in the manner already related.

To finish this part of my subject, it seems only necessary to determine, whether the dependance of visible direction upon the actions of the muscles of the eyes be established by nature, or by custom. But facts are here wanting. As far as they go, however, they serve to prove, that it arises from an original principle of our constitution. For Mr. Cheselden's patient saw objects single, and consequently in the same directions with both eyes, immediately after he was couched; and persons affected with squinting from their earliest infancy, see objects in the same directions with the eye they have never been *accustomed* to employ, as they do with the other they have constantly used.

Having thus shown in what directions external bodies are seen, when their situation with respect to the eye is given, and upon what circumstance the various directions depend, in which a picture upon any one place of the retina can exhibit the object producing it; I should render the theory of visible direction complete, were I now to point out the relative positions of the two lines of direction, in which any two different parts of the retina represent their objects. To ascertain this, the first step must be, to find the place of the retina which receives the picture of an object, whose situation with respect to the external eye is known;

and if two such points of the retina were determined, I think the chief difficulty in this matter would then be overcome. But as it appears to me, that the structure of the eye has not yet been sufficiently explained, to enable any person to take this first step, I forbear saying any thing more upon the subject.

EXPERIMENTS AND OBSERVATIONS

ON

SEVERAL SUBJECTS IN OPTICS.

ARTICLE I.

On Visible Position, and Visible Motion.

IN the estimates we make by sight of the situation of external objects, we have always some secret reference to the position of our own bodies, with respect to the plane of the horizon; and from this cause, we often judge such to be at rest, whose relative places to us are continually changing; and others to be in motion, though they may constantly preserve, in regard to us, the same distance and direction. To give an instance, let us suppose our eyes first directed to a star near to the horizon; should we afterward, by a mere motion of the head, point them to another, some degrees above the former, this second star will appear higher than the first did. Were we now, while the eyes are kept fixed in relation to the head, and the head in relation to the shoulders, to incline the trunk of the body backward, until we bring the optic axes to a third star, this will appear still higher than the second was perceived to be. If instead of directing the eyes successively to different objects, the same object be suffered to remain at the concurrence of the optic axes in all these different positions of the body, it is evident, that it must be seen to move, during the change from one position to another.

The facts I have mentioned are so obvious, that I should not have spoken of them, had I not intended they should introduce the following question: What is there within us, to indicate these positions of the body? To me it appears evident, that since they are occasioned and preserved by combinations of the actions of various voluntary muscles, some feeling must attend every such combination, which suggests, from experience perhaps, the particular position produced by it. But in almost all the positions of the body, the chief part of our muscular efforts is directed toward sustaining it against the influence of its own gravity. Each position, therefore, in which this takes place, must be attended with a feeling, which serves to indicate its relation to the horizontal plane of the earth; and consequently, if our bodies possessed no gravity, or, if the thing were possible, had we been created unembodied spirits, but with the same faculties of perception as we enjoy at present, we could no more have judged one line to be perpendicular, and another to be parallel to the horizon, than we can at present determine, without some external aid, which is the eastern, and which the western point of the heavens. I shall now draw from these principles, the explanation of a fact, which was first mentioned by one of the most ingenious authors that have written upon vision, but left by him still to be justly accounted for.

"I have frequently" (says Mr. Melvill)* "observed, when at sea, that, though I pressed my body and head firmly to a corner of the cabin, so as to be at rest in respect to every object about me, the different irregular motions of the ship, in rolling and pitching, were still discernible by sight. How is this fact to be reconciled to optical principles? Shall we conclude that the eye, by the sudden motions of the vessel, is rolled out of its due position? Or, if it retains a fixed situation in the head, is the perception of the ship's motion, owing to a vertigo in the brain, a deception of the imagination, or to what other cause?"

* Edinburgh Physical Essays, vol. ii. p. 80.

I need not, I believe, offer to show, that the fact here spoken of, is not owing to any of the causes Mr. Melvill has specified. I shall therefore, in a few words, point out its dependance upon the principles which have just been mentioned.

It is generally known, I suppose, that when a vessel at sea, in the language of sailors, is said to pitch, its two extremities turn upon its shorter axis, and that the term of rolling is confined by them to its motions upon the longer axis. In both pitching and rolling then, the relative position of a vessel to a horizontal plane is necessarily changed. Consequently, though, in the abovementioned experiment, Mr. Melvill's body and head were at rest with respect to every object about him, still a different degree of muscular effort was required to keep them so, in every such different position of the vessel. But each degree of muscular effort, to sustain his body against the operation of its gravity, would suggest to him its concomitant position with regard to the plane of the horizon; each deviation, therefore, of the vessel from its former situation, relatively to the same plane, would be perceived, and the vessel itself be seen to move. In short, nothing more takes place in this, than in the following experiment: Let a pole be placed upon firm ground, at right angles to the horizon. If, while we are standing erect, it be inclined upon its lower extremity, successively backward and forward, to the right and to the left, these motions must, without contradiction, be perceived. Suppose now, our bodies to be similarly inclined with the pole, during its different positions, so as to be constantly parallel to it; it is evident, that its motions will be as readily perceived in this case, as they were, when our bodies were erect; and this is all that happens in the experiment of Mr. Melvill.

Should the necessity of supporting the body against its gravity, by the actions of our voluntary muscles, be suspended in whole, or in part, our judgments of the situation of objects, with respect to the horizon, must become irregular and uncertain, notwithstanding any general habit we may have acquired from experience. An instance of this, I think, I have observed; for I have frequently remarked during a sea voyage, that, when the wind blew so strongly, and in such a direction, as to occasion the vessel to heel, or lean much to one side, chords freely suspended from the roof of the cabin, and kept stretched by heavy bodies attached to them, have appeared to me, as long as I lay in bed, though they were necessarily perpendicular to the horizon, to decline considerably from that position; while the sides of the cabin seemed, if not perpendicular, at least much less inclined to the horizon than they were in reality. My body being here supported by the bed, I was consequently without those feelings, which indicate its position with respect to the horizon. Objects therefore appeared to me in those situations, in which I had been accustomed to see them. In confirmation of which I may mention, that, when I got up, and stood upon the floor of the cabin, the chords seemed perpendicular, or nearly so, and the sides of the cabin inclined; for I was now obliged to exert a proper degree of muscular force, to keep myself upright. What I here say, however, is from the recollection of things observed some years ago, when I had no thought of making the use of them I now do; for which reason, I may possibly have committed some trifling error in stating them; but none, I believe, sufficient to affect the theory they are brought to support.

It being my intention to treat, in the present article, of several facts relative to visible position and motion, which seem to me to need explanation, without regarding whether or not they depend upon any common cause; I pass to the consideration of the apparent rotation of objects, when we have become giddy, by turning ourselves quickly and frequently round.

Some of the older writers upon optics imagined the visive spirits to be contained in the head, as water is in a vessel, which therefore, when once put in motion by the rotation of our bodies, must continue in it for some time after this has ceased; and to this real circular movement of the visive spirits, while the body is at rest, they attributed the apparent motions of objects in giddiness. Dechales* saw the weakness of this hypothesis, and conjectured, that the phenomenon might be owing to a real movement of the eyes, but produced no fact in proof of his opinion. Dr. Porterfield†, on the contrary, supposed the difficulty of explaining it to consist in showing, why objects at rest appear in motion to an eye which is also at rest. The solution he offered of this representation of the phenomenon, is not only extremely ingenious, but is, I believe, the only probable one which can be given. It does not apply, however, to the fact which truly exists; for I shall immediately show, that the eye is not at rest, as he imagined. The last author, I know of, who has touched upon this subject, is Dr. Darwin‡. His words are, "When any one turns round rapidly on one foot till he

* Cursus Mathemat. Tom. ii. p. 422.

† Treatise on the Eye, Vol. ii. p. 426.

‡ Philosoph. Transact. Vol. lxxvi. p. 315.

becomes dizzy, and falls upon the ground, the spectra of the ambient objects continue to present themselves in rotation, or appear to librate, and he seems to behold them for some time in motion." I do not indeed pretend to understand his opinion fully; but this much seems clear, that, if such an apparent motion of the surrounding objects depends, in any way, upon their spectra, or the illusive representations of those objects, occasioned by their former impressions upon the retinas, no similar motion would be observed, were we to turn ourselves round with our eyes shut, and not to open them till we became giddy; for in this case, as the surrounding objects could not send their pictures to the retinas, there would, consequently, be no spectra to present themselves afterward in rotation. But whoever will make the experiment, will find, that objects about him appear to be equally in motion, when he has become giddy by turning himself round, whether this has been done with his eyes open or shut. I shall now venture to propose my own opinion upon this subject.

If the eye be at rest, we judge an object to be in motion when its picture falls in succeeding times upon different parts of the retina; and if the eye be in motion, we judge an object to be at rest, as long as the change in the place of its picture upon the retina, holds a certain correspondence with the change of the eye's position. Let us now suppose the eye to be in motion, while, from some disorder in the system of sensation, we are either without those feelings, which indicate the various positions of the eye, or are not able to attend to them. It is evident, that, in such a state of things, an object at rest must appear to be in motion, since it sends in succeeding times its picture to different parts of the retina. And this seems to be what happens in giddiness. I was first led to think so from observing, that, during a slight fit of giddiness I was accidentally seized with, a coloured spot, occasioned by looking steadily at a luminous body, and upon which I happened at that moment to be making an experiment, was moved in a manner altogether independent of the positions I conceived my eyes to possess. To determine this point, I again produced the spot, by looking some time at the flame of a candle; then turning myself round till I became giddy, I suddenly discontinued this motion, and directed my eyes to the middle of a sheet of paper, fixed upon the wall of my chamber. The spot now appeared upon the paper, but only for a moment; for it immediately after seemed to move to one side, and the paper to the other, notwithstanding I conceived the position of my eyes to be in the mean while unchanged. To go on with the experiment, when the paper and spot had proceeded to a certain distance from each other, they suddenly came together again; and this separation and conjunction were alternately repeated a number of times; the limits of the separation gradually becoming less, till, at length, the paper and spot both appeared to be at rest, and the latter to be projected upon the middle of the former. I found also, upon repeating and varying the experiment a little, that when I had turned myself from left to right, the paper moved from right to left, and the spot consequently the contrary way; but that when I had turned from right to left, the paper would then move from left to right. These were the appearances observed while I stood erect. When I inclined, however, my head in such a manner, as to bring the side of my face parallel to the horizon, the spot and paper would then move from each other, one upward and the other downward. But all these phenomena demonstrate, that there was a real motion in my eyes at the time I imagined them to be at rest; for the apparent situation of the spot, with respect to the paper, could not possibly have been altered, without a real change of the position of those organs.

To have the same thing proved in another way, I desired a person to turn quickly round, till he became very giddy; then to stop himself and look stedfastly at me. He did so, and I could plainly see, that, although he thought his eyes were fixed, they were in reality moving in their sockets, first toward one side, and then toward the other.

The last instance of visible motion I shall notice, is one which has been mentioned by Mr. Le Cat, in the following words*: "Place a lighted candle at a moderate distance from a polished body of considerable convexity, so that the image of the flame, which is seen by reflection from it, may appear as a small luminous point. The experiment will succeed better, if the direct rays of the flame be intercepted from the sight. Close, after this, one eye, and view the luminous point in a careless way, (*en revant*) that is to say, with the eye in a relaxed or dilated state. The point will then be seen enlarged and radiated. If you bring now your finger to the right of the eye which is open, and gradually move it toward the left, in order to conceal the luminous point from this eye, you will distinctly perceive the shadow of your finger to proceed from left to right, and to pass over the point in

* Traité des Sens. p. 419.

a direction, contrary to that which you gave it. Should you, afterward, move your finger back from right to left, and in like manner, if your finger be moved from above downward, or from below upward, the shadow will always proceed the contrary way. It is therefore manifest, that the soul must here see objects inverted, as their images in the eye truly are; and that it refers impressions to those parts of the eye where it feels them, and not to the places from which the rays are emitted, as it does when it possesses the means of rectifying its judgment. Whence does this happen? Doubtless, because the luminous point has neither a high nor a low, neither a right nor left side, nor any well-enlightened object in its vicinity, to awaken the attention of the soul; in short, nothing which can determine its judgment."

I should scarcely have mentioned this experiment, from any respect for the authority of its author in optics; but as Haller* seems to assent to the conclusion he draws from it, that the soul sometimes sees objects inverted; and as the Abbot Derochon†, a member of that learned body, the Academy of Sciences of Paris, has lately, but in my opinion unsuccessfully, attempted to reconcile it to the commonly received

* Elementa Physiologiæ, Tom. v. p. 479.

† Memoires de Physique, p. 66.

principles of vision, I think it worth while to show, in a few words, that it is a direct consequence of the very doctrine Mr. Le Cat means to overthrow by its means.

It would be proper, indeed, to mention beforehand, the opinion of the Abbot Derochon; but this I must, notwithstanding, omit doing, as it could not be understood without the figure by which he has illustrated it. I shall observe, however, respecting it, *first*, that it requires the side of the finger next to the eye, to be without the least illumination; whereas the experiment will succeed, whether it be illuminated or not: *secondly*, that, according to it, the experiment ought to succeed equally well, whether the image of the flame in the mirror be seen as a point, or as a surface; though, in truth, it never does succeed, except in the latter cases: *thirdly*, that the apparent shadow of the finger is always much larger than it ought to be, were it seen by reflection, as the Abbot thinks: *fourthly*, that, while the eye, mirror, flame, and finger, remain in the same positions, the shadow seems at one time larger than at another, owing to the different degrees of relaxation in the eye; but that this, for the reason just mentioned, ought never to happen, according to his theory: *fifthly*, that agreeably to his own reasoning, the shadow ought to move in the same direction with the finger, which is the very reverse of the fact to be explained. But as arguments against error may be infinitely extended, and as only one solution of a phenomenon can be true, the readiest way of exposing the insufficiency of others, is to exhibit that which is just.

This, in the present case, seems to lie upon the very surface of optical knowledge, and has already been given by others, of various forms of the same fact. When the image of the flame is seen in the mirror as a point, its rays must be accurately collected to a focus in the retina; but when seen as a surface, this must necessarily be attributed to their focus being either before or behind it; in either of which cases, they will occupy a place upon that membrane of some assignable dimensions. In the present instance, their diffusion over a part of the retina, depends on the focus being behind it; for the eye is now, from a condition of the experiment, in a more relaxed state than it was just before, when the rays of the same object were brought there accurately to a point. The rays, therefore, which go to the right side of the enlightened surface of the retina, or picture as I shall call it, are those which enter the eye at the right side of the pupil, and its left side is formed of the rays entering at the left side of the pupil; and the like must be true of its upper and lower parts. Should we then begin to move a finger from right to left across the eye, the rays forming the right side of the picture must be first intercepted. But from the known fact, that the points of an external object are always in an inverted position, with respect to the parts of the retina, by the affections of which they are suggested, when the *right* side of the picture there is effaced, the *left* side of the external object it suggests must disappear. And for the same reason, if the motion of the finger be continued from right to left across the eye, the other parts of the luminous surface in the mirror will successively vanish from left to right, and thereby furnish the appearance of a shadow passing over it in that direction.—In like manner, it may be shown, that if the finger proceeds from left to right, from above downward, or from below upward, the shadow must move the opposite way.

That this is the true explanation of Mr. Le Cat's experiment, is, I think, plain, both from its intrinsic evidence, and the following considerations:—If the mirror be brought within four or five inches of the eye, and the candle be so placed, that the image of the flame must, from the laws of reflection, be regarded as a mere point; though we should now view it with the utmost care, and though there should be in its neighbourhood some well-enlightened object to awaken the attention of the soul, as Mr. Le Cat expresses it, still the seeming shadow will move in a direction contrary to the finger. For the image is now so near to the eye, that no exertion we can make is sufficient to bring its rays to a point upon the retina; the picture, therefore, upon that membrane will be formed of rays passing to a focus behind it, which is the only condition necessary for the success of the experiment. Again, if a short-sighted person should place the mirror at the distance of some feet from him, complying in other respects with Mr. Le Cat's instructions, he will constantly observe the shadow to move in the same direction with the finger. For, in his eye, the rays of the image, when at such a distance, must meet before they fall upon the retina. The right side, therefore, of the picture upon that membrane, must be composed, in this case, of rays which enter the eye at the left side of the pupil. Consequently, when these are cut off, the left side of the apparent luminous surface must disappear, and the shadow be seen to move the same way as the finger, when this successively intercepts the rays proceeding from the image to the eye*.

* Scheiner observed a fact of the like kind (Fundamentum Opticum, p. 33) namely, that, if a small hole, made in any substance, be held near to the eye, and an opaque body be passed between them, from right to left, the left side of the hole will first disappear. Mr. Grey afterward took notice (Philosoph. Transact. Vol. xix. p. 286) that a needle he employed in this experiment was seen inverted; from which he supposed that the hole, or something in it, produced the effect of a concave speculum. Mr. Harris, however, says (Treatise of Optics, p. 141) that it is not the needle, but its shadow on the other side, which is seen, and is the cause of the inverted appearance. But the truth is, that the hole is to be regarded as a luminous point, the rays of which fall upon the retina before they are collected to a focus; and hence that the same appearances must be here observed as in the experiment of Mr. Le Cat. In proof of this it may be mentioned, that if the hole be placed at such a distance, that the eye may refract its rays accurately to a point on the retina, no shadow or image of the needle will be seen; that if the hole be still farther removed, and the eye be adapted to a less distance, the shadow or image will again appear, but its position will now be upright, and its motion the same way as that of the needle itself; and lastly, that, at one given distance of the hole, either no shadow will appear, or it will be seen upright, or it will be seen inverted, according as the eye may be made to assume different states with respect to its power of refraction.

ARTICLE II.

On a supposed Consequence of the Duration of Impressions upon the Retina; and the Effects of accurate Vision being confined to a single Point of that Membrane.

Few things, at first, appear more incredible to a person, not conversant in optics, than that he does not, at any one time, see distinctly a surface larger than the head of a pin. After he is convinced, by proper trials, of the truth of this, he naturally asks, Whence comes it then, that, in ordinary vision, I seem to view distinctly so many objects at once? I go into a crowded street, and I fancy I have an accurate perception by sight, of men, houses, carriages, and many other things, all at the same time; whence proceeds this illusion?

Only one answer, as far as I know, has been given to this question. The impressions made upon the retina by external objects, do not, it is said, immediately cease, along with the reception of the rays which flow from them; and, as in the ordinary mode of vision, the eye is continually passing from object to object, the impression left by a former one may be still vivid, though the eye be directed to another;

and hence we may imagine we see both of them distinctly, though the picture of only one occupies that place of the retina, which alone furnishes us with accurate vision.

There are, however, objections to this answer, which seem to me insurmountable. For, *in the first place*, as the duration of impressions on the retina must be greater or less, according to the vivacity of the pictures which occasion them, it follows, that, were this answer just, the apparent field of our distinct vision ought to be in proportion to the quantity of light admitted by the eye; that it should be contracted, therefore, by every cloud which passes over us, and be enlarged by every burst of sunshine; that, at mid-day, it should possess its greatest extent, and ought from that time gradually to decrease till the evening, when its limits should be nearly the same with those of the real field of accurate vision. *Secondly*, since the coloured spot, which is produced by looking steadily for some time at a luminous body, appears projected upon every object to which we direct our eyes, during its continuance, and as such a spot is necessarily the sign and effect of the duration of an impression upon the retina; every other visible appearance from the same cause ought, in like manner, to have its situation determined by the position of the eye, as far as this may be occasioned by the action of its muscles. No object, therefore, ought to appear separate and distinct from others, if the answer were true which I am combating; but, on the contrary, all those to which we successively direct our eyes during the limits of the duration of an impression upon the retina, should seem crowded into one place; and, consequently, none of them should be perceived with any tolerable accuracy.—Such are the conclusions from the truth of this answer. I need scarcely mention, that they are contradicted by experience.

There is another form of the same fact, to which, it may be thought, an explanation taken from the duration of impressions on the retina will better apply; I mean the appearance of a fiery circle, when any red-hot body is moved quickly round. But it seems, to me, that such an explanation cannot even here be admitted. For, if the circle depended upon the cause I have mentioned, it could only be observed as long as the impressions upon the retina were also disposed in the form of a circle. Were this broken upon, which it must be by every movement of the eye, the appearance suggested by the last impression would no longer be so arranged, with respect to the appearance suggested by the present impression, as to lie with it in the circumference of a circle; and hence some very different figure would be observed. Every person, however, may easily convince himself, that the circular form of the fiery appearance is equally perceived, whether the eye be at rest, or be moved in the most irregular manner.

If these arguments be thought sufficient for the purpose I had in view, it must also follow from them, since the fact still remains to be explained, why we apparently see so many objects with equal distinctness at once, that past impressions upon the retina are perceived as present, by means of some higher faculty than that of sight. This faculty cannot, with propriety, be named *memory*, as it is essential to a thing's being remembered, that it be perceived as past. Nor can it be called *imagination*, since we believe in the present existence of what it perceives. In one point of view it may seem rather a defect in our natures, that we should not be able to distinguish between things past and present. However this may be, I am inclined to be of opinion, that many other phenomena, both of thought and external sense, are partly to be resolved into the same general fact. From the present instance of it, we learn, that several muscular actions may be performed, in succession, during the least perceptible portion of time.

The question I have just treated, naturally gives rise to another: Would it have been more to our advantage, if accurate vision, instead of being confined to one point of the retina, had been possessed by every part of that membrane? I answer, I think not, for the following reasons.

First; The diffusion of such a property over the whole retina would be of little use, unless our power of attention was also increased. For we should otherwise be still unable to perceive more than one visible object at once, with distinctness, since, by our present constitution, we are capable of attending accurately to only one thing at a time. The only benefit, indeed, I can see to arise from such a condition of the retina, is this; That our attention might be shifted more quickly from picture to picture on that membrane, than our eyes can be turned from one external object to another. This advantage, however, would be far out-weighed by an inconvenience accompanying it. For it is a well-known fact, with respect to perception, that we are capable of attending, more or less accurately, to any particular impression upon the senses, in proportion to the inferior force of other impressions, which are at the same time received. But in the supposed state of the retina, there would be, almost always, several impressions of the same strength as the one to which we might desire particularly to attend; whereas, in its present state, the vivacity of the impression from the object, to which we turn the optic axis, most commonly surpasses, considerably, that of every other upon the same membrane; by which means our attention is rendered less liable to interruption.

Secondly; The extension of accurate vision, to every part of the retina, would deprive us, in great measure, of the help, which we obtain, at present, from the eye, in learning the thoughts of other men. As far as I have been able to observe, the changes produced by our internal feelings, upon the state of the eye itself, are very few, and relate only to the quantity of moisture, which is diffused over its surface, and the degree of fulness in the blood-vessels, which are spread upon its white and glistening part. Both of these circumstances, however, are similarly altered by opposite passions, and, consequently, neither of them can be regarded as the appropriate expression of any. The whole variety, then, of the expressions of feeling which are justly attributed to the eye, must, I think, depend upon its motions. Some of these are the immediate effects of certain passions; the eye, for instance, being moved differently in anger and in grief; and such may be esteemed as directly expressive of the passions by which they are produced. But the far greater number of them do little more, than merely point out the external cause, or object of the sentiment, which the changes of other parts of the countenance declare to exist within us; or distinguish certain external appearances depending upon a mental cause, from similar appearances arising from a different source. Thus, blushing is often distinguished from an accidental flush of the cheek, by the eye being turned away from the person who occasions it.

That many of the expressions, which we attribute to the eye, do in fact depend on changes in other parts of the countenance, is evident from the alterations we think induced upon it, by the eyelashes falling off from disease, by a slight inflammation of the edges of the eye-lids, without its being communicated to the eye itself, by artificially colouring the eye-brows, and by many other similar circumstances. And how essential to the right understanding of the expressions of the other features, are the motions of the eyes, when conducted with design, and properly directed, must be known to every one, who has attended in discourse to the countenances of very short-sighted people, and more especially to those of persons afflicted with blindness from a *gutta serena*, in which the eye, with respect to its external condition, seems without fault. But whatever is the assistance the motions of the eye afford, in expressing our internal feelings, the whole of it must ultimately be referred to the circumstance of accurate vision being confined to one point of the retina; since the intent of those motions is, to bring the pictures of external objects upon the most sensible part of that membrane. Their necessity, therefore, would no longer exist, if the same property were extended, and the advantages we at present enjoy from them, would, consequently, cease.

ARTICLE III.

On the Connexion between the different refractive States of the Eyes, and the different Inclinations of the Optic Axes to each other.

I HAVE mentioned, in my Essay upon Single Vision with Two Eyes*, that I had been convinced, by experiments almost without number, that every different degree of the mutual inclination of the optic axes, is attended by a different state of the refracting power of each eye. The experiments I there alluded to were chiefly of this sort. I placed a luminous point, most commonly the reflected image of the flame of a candle from the bulb of a small thermometer, at such a distance, that when both my eyes were accurately directed to it, its visible appearance to one of them was likewise that of a point. Keeping then the axis of this eye fixed, and making the other to cross it, sometimes before and sometimes behind the luminous point, I found that in both cases it appeared as a surface to the eye, in the axis of which it was

* Page 66.

situated; and that the more remote from it was the concurrence of the axes, the larger was the luminous surface. Now when the axes met before the point, the apparent surface must have been occasioned by the rays coming to a focus, previously to their incidence upon the retina; because, when I passed my finger across the eye by which it was seen, its parts disappeared, in an order corresponding to the direction in which the finger moved. The disappearance of the parts was in an order, contrary to the motion of the finger, when my optic axes intersected each other beyond the point; which is an equal proof, that the rays, in that case, tended to a focus behind the retina.

One application of this fact has already been shown*, and I shall now proceed to mention several other phenomena in vision, which it may serve either in whole, or in part, to explain.

1. It accounts for the following beautiful observation made by Aguilonius†, that if we close one eye, and look with the other at an object placed in its own axis, we shall not be able to see this object distinctly, unless we also direct to it the axis of the closed eye. For in persons, who are neither presbytic nor myopic,

* Essay upon Single Vision, p. 66.

† Aguilonii Optica, page 84.

the refractive states of the eyes are so adapted to the mutual inclinations of the optic axes, that pencils of rays flowing from bodies at moderate distances are more accurately collected upon the retina, when they are situated at the intersection of those lines, than if their position was, in any considerable degree, either nearer or more remote. The reason given by Aguilonius himself, is, that the mind perceives only those objects distinctly, which are placed at the concourse of the optic axes. But the following experiment proves that the solution is true no farther, than as it coincides with the one I have advanced. Hold, in the axis of either eye, a concave lens, at such a distance, that the letters of a book, placed a little farther off, may appear through it very indistinct to that eye, when both axes are directed to any particular word. View afterward the lens itself with both eyes, and the letters will immediately become more distinct. In this experiment then, an object is more accurately perceived when distant from the concourse of the optic axes, than when situated exactly in it.

It may be said, perhaps, that the distinctness of the letters is here to be attributed to the contraction of the pupil, which is occasioned by the eyes being directed to a nearer object than they were formerly. But that this is not the case, may be made evident by another experiment: Place a convex lens in such a manner before one eye, that the flame of a candle, at the distance of two or three feet from the face, may appear indistinctly terminated to that eye, when both axes are pointed to it. The same eye being kept fixed, let the two axes afterward meet beyond the flame, and it will now be seen much better defined, though the pupil is at the same time become larger. The insufficiency of the explanation of Aguilonius, is also proved, by a circumstance frequently noticed in persons who are very short-sighted; for such are observed, when they desire to view an object with much attention, to hold it close to one eye, and to turn the other aside; in this way occasioning the two axes to meet very remotely from the object.

2. The reason commonly given, why short-sighted people view an object with one eye only in the manner above-mentioned, is, that by this means they avoid the uneasy straining of the muscles, which must be employed to direct both axes to the same point. But it is evident they must derive from the practice this farther advantage, that, as their optic axes are now parallel to each other, or nearly so, they, consequently, see the object in the least refractive state of their eyes. Pencils, therefore, will

now have their focuses in the retina, the rays of which would have crossed each other, before they fell upon it, had both the axes been directed to the object.

3. Spectacles were long employed, before the manner in which they assisted sight was known. About the year 1601, this was proposed as the subject of a question to Kepler,* by his principal patron at that time, Ludovic L. B. a Dietrickstein, a learned nobleman of Austria. The first answer he gave was, that convex glasses were of use, by occasioning objects to appear larger. But his patron observed, that if objects were rendered by them more distinct, because larger, no person would be benefited by concave glasses, since these diminish objects. It was not till three years after, that, in consequence of finding out in what manner vision is performed, he was able to give a just solution of this problem, though his attention had been directed to it during the whole of that interval. According to the discovery he then made, convex glasses were said by him to assist the sight of presbytic persons, by so altering the directions of rays diverging from a near object, that they shall afterward fall upon the eye, as if they had proceeded from a more remote one; and

* Paralipomena in Vitellionem, p. 200.

concave glasses to benefit the myopic, by producing a contrary effect upon rays which diverge from a distant object. Now it is manifest, that by this theory, to which I believe no addition has been made by any succeeding writer, precisely the same effects are attributed to lenses, whether they be employed singly, or in the form of spectacles. I am inclined, however, to think, that a difference, sometimes at least, exists here, which has hitherto escaped notice. For in regard to such spectacles as I have tried upon myself, I have always found, that, when I looked with them at objects placed at moderate distances directly before me, my optic axes passed through the glasses, more inwardly than their centres. With respect, therefore, to spectacles for long-sighted people, as the inner halves of their glasses may be regarded as two prisms, whose refracting angles face each other, to have allowed both my eyes to receive through them pencils of rays from the same point of an object, the intervals of my pupils must have been less than was necessary for that purpose in naked vision. The consequence of which would be, an increase of the refractive power of my eyes. Again; as the like parts of glasses in spectacles for short-sighted persons, may be esteemed to be two prisms, the refracting angles of which are turned from each other, the interval of the pupils must have been increased, and the refracting power of my eyes by this means diminished, when I looked at an object through them, which was directly before me. And effects similar to what I have mentioned, must have followed my viewing objects placed obliquely, through glasses of both kinds. Here then is one advantage, which persons, who see with both eyes, either do or may enjoy from spectacles, but which they cannot derive from using single glasses. For if they are presbytic, they can see an object by the means of them with a higher refractive state of the eyes, than if the optic axes met there, as in naked vision; and if myopic, with a less. It is also worthy of remark, that this advantage does not ultimately tend to increase the evil, which first gives occasion for spectacles. On the contrary, if what every writer upon vision asserts be true, that we are apt to become short or long-sighted, according as we are much accustomed to view near or distant objects, it must serve to diminish that evil. In support of this opinion, I shall mention a fact, with which I have been made acquainted by Mr. George Adams*, of this place, who is not only well

* Mathematical Instrument Maker to the King.

skilled in the theory of vision, but, from his situation, as an artist, has better opportunities, than most persons, of learning such matters. The fact is this, that he does not know a short-sighted person, who has had occasion to increase the depth of his glasses, if he began to use them in the form of spectacles; whereas he can recollect several instances, where those have been obliged to change their concave glasses repeatedly, for others of higher powers, who had been accustomed to apply them to one eye only. This indeed may have happened by accident; but at any rate, the fact is worthy of farther attention and inquiry.

It would seem, however, that the long-sighted derive more benefit from the alteration in the mutual inclinations of the optic axes, which is produced by spectacles, than the short-sighted. For, as the inner halves of the convex glasses are to be regarded as prisms, with their refracting angles continually increasing as we approach their edges, if two objects, situated at different distances, be viewed successively through them, the inclination of the optic axes to each other, when the nearer object is seen, must bear a higher proportion to their inclination, when we look at the one more remote, than the different inclinations of the optic axes do to each other, when they are successively directed to the same objects, without the intervention of such glasses. Hence the nearer the object is, the greater will be the effect of the variation in the inclination of the axes produced by spectacles with convex glasses; which is the order of things, the best adapted to the wants of those who use them. But with respect to short-sighted persons, since the refracting angles of their glasses, considered as prisms, *decrease*, in proportion as the objects seen through them become more remote; they must, consequently, derive the least benefit from an alteration in the mutual inclinations of the optic axes occasioned by their spectacles, at the time they most require it.

If it were asked, then, what is the real foundation of the common reproach against spectacles for long-sighted people? I should answer, a very different one from that, which is, for the most part, assigned.—For the change, in the conformation of the eyes, which renders them useful, seems to be one of those which nature has destined to take place at a particular age, and to which there is no gradual approach through the preceding course of life. A person, for instance, at forty, sees an object distinctly, at the same distance that he did at twenty. When he draws near to fifty, the change I have spoken of commonly comes on, and obliges him in a short time to wear spectacles. As it proceeds, he is under the necessity of using others with a higher power. But, instead of supposing that his sight is thus gradually becoming worse, from a natural process, he attributes the increase of the defect in it to his too early and frequent use of glasses. Upon the whole, I should draw this inference from what has been said, that no person, whose sight begins to grow long, ought to be, in the least, prevented from enjoying the immediate advantage which spectacles will afford him, by the fear that they will ultimately injure his eyes; not that I think the convexity of each glass, considered by itself, can do no harm, but that I believe the benefit, arising from the combination of the two, to be at least sufficient to compensate it. Whether those, who have a tendency to short-sight, should be also early in their employment of spectacles, I shall not pretend to say; as there is not the same ground, from theory, for supposing, that the benefit arising from the combination of the two glasses is able to over-balance the injury, produced by the concavity of each considered separately.

All that I have said, however, upon the subject of spectacles, proceeds upon the supposition, that, when objects, placed directly before us, at moderate distances, are viewed through them, the optic axes penetrate the glasses more inwardly than their centres. But I can be by no means sure, that the interval of the pupils of other persons, bears the same proportion to the interval of the centres of the lenses in spectacles, as that of mine does. It concerns those, therefore, who are choosing them, to have attention to this circumstance. To me it appears proper, that the glasses in spectacles, both for long and short-sighted people, should be so far asunder, that, when we look at a very remote object directly before us, our optic axes may pass exactly through their centres. For if the centres of convex glasses be nearer to each other, very remote objects will appear double; and if they are more distant, though the object viewed be infinitely far from us, the optic axes will, however, be inclined to one another, and the refractive power of the eyes increased, when this may be of disservice; since there are few eyes which are not able, even without the aid of the convexity of a glass, to bring parallel rays to a focus upon the retina. If the centres of lenses in spectacles, for the short-sighted, be less distant than what I have mentioned, the optic axes must be bent toward each other, when very remote objects are seen, and the refractive state of the eye, therefore, heightened, which is the very reverse of what is here to be desired. Should the interval of the centres of those lenses be greater, objects at very considerable distances will be seen double.

There are two other observations relative to glasses for the sight, which I wish to add to what I have already said upon this subject. The *first* is, that the single convex glasses with which some persons read, must be very injurious, if they be sufficiently large, to admit the same object to be seen with both eyes. For as both axes will then pass through them, one on each side of the centre, the interval of the pupils will be widened, and the refracting power of the eyes be diminished; so that here a disadvantage is to be added to the prejudice of the convexity of the glass, not a benefit to be placed against it, as in the case of common spectacles for the long-sighted. If, indeed, the defect in sight does not arise from the conformation of the eye, but from a want of transparency in its cornea or humours, then such glasses, by magnifying objects, will be useful, for the same reason, that, in a very faint light, we can read a book of a large print, with more ease than one of a smaller. The *second* observation is, that if flat-sided prisms were fixed in spectacle-frames, with their refracting angles toward each other, they would assist the long-sighted somewhat, without producing the evil which is said to arise from the convexity of lenses; and spectacles of this kind might, with more propriety, I think, than any others, be called *preservers*. A like combination of such prisms, but with their angles turned the other way, might, when the object was moderately distant, be of service to the short-sighted. But objects, very remote, would be made by them to appear double.

ARTICLE IV.

On the Limits of perfect or distinct Vision.

Dr. Jurin*, I believe, was the first who distinguished between *perfect* and *distinct* vision; confining the former term to those cases, where the rays of a single pencil are collected to a single point of the retina; and marking, by the latter, the perception we have of visible objects, when the rays of the pencils, diverging from them, though not collected to single points of the retina, yet occupy so small portions of it, as to allow the objects to be distinctly seen. But as few authors have adopted this division, I shall, in the present article, use both terms in the sense, which he has appropriated to the first. Neither of them is indeed free from objection, since bodies to be distinctly or perfectly seen, not only require, that their pictures should be accurately formed upon the retina, but that they should fall upon a particular part of it.

Although it has long been a subject of inquiry, within what limits of distance objects are distinctly perceived by sight, yet the only

* Essay on distinct and indistinct Vision.

experiments I have met with in books, which have been made, with any tolerable show of accuracy, to determine this matter, are those of Dr. Porterfield. I shall not here say what they are, as his Treatise is in every body's hands, but shall only mention the principal conclusions which he drew from them; *first*, that objects could be distinctly seen by him, that is, the pencils of rays which came from them could be accurately collected to points upon the retina, when their distances from his eye did not exceed twenty-seven inches, and were not less than seven; and *secondly*, that, as often as the axes of both eyes were directed to any one point, situated within those distances, the rays proceeding from it had their focus in each retina.

As the results of some experiments, which I have made upon the same subject, differ from these conclusions of Dr. Porterfield, I have read over what he has written upon the matter with more than ordinary attention, and I think I can thence show reason, why they should not be received without caution. For, *in the first place*, his experiments are related so circumstantially, and with such an appearance of accuracy in the making of them, that you would scarcely suppose he left the least possible room for error. And yet, after finishing his account of them, he tells us, that he would have repeated them *with more care and exactness**, had he not been interrupted. *Secondly*, his experiments were made upon one eye only, though his conclusions apply to both eyes; an inaccuracy which gives occasion to suspect others. *Lastly*, he says, that he could not see an object distinctly at the distance of seven inches, unless both axes were pointed to another object, at only half that distance. Had he then directed both axes to an object seven inches distant, which he does not mention he ever did, it must consequently have been seen *indistinctly*; and yet one of his conclusions states, that objects, distant from about seven, to about twenty-seven inches, were always *distinctly* seen, when the axes of both eyes were directed to them. Such are the reasons which lead me to think, that the whole of the difference, between the results of the experiments of Dr. Porterfield and myself, is not to be attributed to a difference in the structure of our eyes.

The experiments, which I made upon this subject, were with luminous points. They proved to me, *first*, that, when both optic axes are directed to any object, placed at a less distance from my eyes than about seventeen

* Treatise on the Eye, Vol. I. p. 423.

inches, my vision of it by the left eye is indistinct, from the rays of light tending to focuses behind the retina; *secondly*, that my vision by the same eye is perfect, if the object seen, and to which both axes are turned, be from about seventeen to about nineteen inches distant; *thirdly*, that the vision of my left eye becomes again imperfect, if the object be moved to a greater distance than that of nineteen inches, the rays being now collected to focuses, previously to their falling upon the retina; and *fourthly*, that I have, by my right eye, imperfect vision of all objects, to which I direct both axes, unless their distances be so great, that the rays of each pencil, proceeding from them, may be regarded as parallel.

A conclusion is furnished by these experiments, similar to one, which was drawn by Mr. Delahire *, from some made by himself; namely, that each eye sees objects distinctly only at one distance; as I take for granted, that, in every case of ordinary vision, both axes are directed to the object which is viewed. But Mr. Delahire drew a second conclusion from his experiments, which he seems to have regarded only as another expression of the first, but which, in truth, includes a very different fact. It was,

* Memoires de Mathematique et de Physique, 4to. p. 298.

that the refractive state of the eye is always the same, whether we look at a very near or a very distant object. The following observations, however, will prove the contrary, at the same time that they show, in what I farther differ from Dr. Porterfield.

1. Though an object, to which both axes are pointed, does not appear distinct to my left eye, unless it be from about seventeen to about nineteen inches distant; nor to my right eye, unless it be at a very considerable distance; yet I find, that when the axes are made to meet at a point, about two inches distant from a line connecting the two pupils, which however cannot be effected without much straining, my left eye will now see an object distinctly, which is only about seven inches from it, and my right eye will at the same time see an object distinctly, the distance of which is about ten inches. I find also, that my left eye is made to see an object distinctly, though placed more than nineteen inches from it, if I direct both axes to a point still more remote.

2. I formerly mentioned, that every degree of the mutual inclination of the optic axes is attended, by a particular state of the refracting power of each eye. But I must now remark, that these states are sometimes subject to slight variations, while the inclinations of the optic

axes to each other remain the same. For I find, that, when a luminous point, to which both axes are turned, is distinctly seen by my left eye, I can, by certain efforts not easily to be described, but without changing the position of either axis, make it afterward appear as a surface, and this too, at one time, from the rays coming to a focus too soon, and at another, too late, for perfect vision*. One instance of these variations deserves to be minutely described, as it proves, that the refractive power of the eyes is subject to greater changes, than what are shown by any experiments I have met with in authors. When I look attentively at a bright star, with the optic axes parallel to each other, it appears to my left eye a surface of some extent, and to my right eye, though not a point, yet a surface of very small extent, as small as

* The variations, however, seem produced in such a manner, that the middle of the set belonging to one degree of the mutual inclination of the optic axes, is always different from the middle of the set belonging to another degree of their inclination; and that, when no other effort is made, than to direct both axes to the same object, the eyes always assume the middle state of the refractive power, which accompanies that particular inclination of the axes. No argument, therefore, can hence be derived against the applications I formerly made of the general fact, respecting the connexion of the refractive states of the eyes with the mutual inclinations of the optic axes.

the sphericity of the cornea and crystalline, the various refrangibility of the different kinds of light, and the width of the pupil at night, can be supposed to allow; for I find, that, if I now pass a needle across the axis of the right eye, its shadow will not be seen. But should I, after this, withdraw my accurate attention from the star, and view it in the state of sight we have, when we are said to be in a *reverie*, in which, though our eyes are open, we are yet scarcely conscious of seeing surrounding objects, the appearance to the right eye expands itself, and if a needle be again passed before this eye, its shadow will be observed to move over the star, in a direction contrary to that of the needle itself; a sure indication that the rays of light now tend to a focus behind the retina. In the same state of things, the appearance of the star to the left eye contracts, and if a needle be held before the eye, no shadow is seen; a sign that the rays are collected to a focus on the retina; whereas they had formerly crossed one another before they reached that membrane.

Upon the whole then it is manifest, from the experiments I have related, that my left eye can collect to focuses in the retina, rays which proceed from objects at every distance whatsoever, not less than seven inches; that my right eye can collect to focuses in the retina, rays which proceed from objects at every distance whatsoever, not less than ten inches, and even such as are somewhat convergent, since it can make those, which are parallel, to meet before they fall upon the retina; and *lastly*, that, while both the optic axes are directed to a point within the limits of distinct vision, the rays proceeding from it are never accurately collected to focuses in both retinas, and scarcely ever to a focus in either retina. These are likewise the principal circumstances, in which my experiments differ in their results from those of Dr. Porterfield.

In making such experiments with luminous points, one or other of two appearances very constantly occurs, neither of which, as far as I know, has been spoken of by any preceding author. The most proper way of mentioning what they are, is, perhaps, to show what ought to happen in those situations, in which they are observed.

When a beam of white light passes, obliquely, from one medium into another of different refractive power, its variously coloured rays must begin to diverge from each other, at the point of the beam's incidence upon the latter medium. In achromatic telescopes, the mutual separation of these rays is checked, and its farther increase prevented, before it becomes perceptible to sense, by the contrary refractions which they undergo, from passing, successively, through the different parts of the object-glass. Hence, some have imagined, that, since objects, in ordinary vision, are seen without colour, as far as this depends on the refractions of the eye, nature has furnished us with an instrument, constituted upon principles similar to those of the object-glass of an achromatic telescope. But every one, the least acquainted with the structure of the eye, must know, that this cannot be the case, as the refractions in it are all made one way *. And there are experimental proofs, that compounded light is always separated into its parts, by passing through the eye. For if we interpose any opake substance between us and a luminous body, so that only a very small portion of this may remain visible, it will appear to consist of three differently coloured parts, red, yellow, and blue. The reason, therefore, of objects being, for the most part, seen colourless, must be elsewhere sought †.

Now let us suppose, that a luminous point is the only object which is seen at any one time; should the focus of its mean refrangible rays be

* There are indeed some exceptions to this, but not of sufficient consequence to affect the present argument.

† Dr. Maskelyne has very learnedly treated this subject in the Philosophical Transactions, vol. lxxix. part 2.

anterior to the retina, the middle of its picture upon that membrane must be chiefly composed of the less refrangible rays; and this must be the reason, that, when I look attentively at a bright star with my left eye, the centre of it always appears of a light orange colour. As the beams, however, from the luminous point, which enter the eye near to its axis, suffer but little refraction, the brightness of their white light, will, in great measure, overpower the colour given to the middle of the picture upon the retina, by the less refrangible rays of those, which enter the eye at a distance from its axis. Were you then to intercept the former beams, the effect I have mentioned of the latter, must be more observable; and hence it is, that when I place a pin or needle between my eye and a luminous point, the rays of which come to a focus before they fall upon the retina, the shadow, instead of appearing black, is always of a red or deep orange colour; which is one of the phenomena respecting luminous points, to which I have alluded.

On the other hand, should the focus of the mean refrangible rays of a luminous point lie behind the retina, the middle of the picture there will be principally formed of the more refrangible rays; and if the beams, which enter the eye near to its axis, be also in the present case intercepted, the effect of the latter rays, in giving colour to the middle of the picture, will consequently be rendered more evident. Hence it is, that, when a luminous point is not sufficiently remote for distinct vision, the seeming shadow upon it, occasioned by any small opake object held before my eye, is always blue; and this is the second of the appearances, which I said are frequently to be observed, in experiments upon luminous points.

AN

ESSAY

ON

DEW,

AND

SEVERAL APPEARANCES

CONNECTED WITH IT.

TO

JAMES DUNSMURE, ESQUIRE,

MERCHANT IN LONDON.

MY DEAR SIR,

Without your aid, I should, in all probability, never have acquired the knowledge, upon which the following Essay is principally grounded; since I could not, I believe, have found any other place, considering that I was obliged to be daily in London, so well fitted for my experiments, as that which you permitted me to use during a very long time, though manifestly to the great inconvenience of yourself and your family. I beg leave to assure you, that I feel this kindness most strongly, and that my gratitude for it will never cease to exist.

I am,

My dear Sir,

Your most obedient Servant,

and faithful Friend,

WILLIAM CHARLES WELLS.

London,
August 25, 1814.

The following notice was prefixed by the Author to the second edition, published in 1815.

The infirm state of the Author's health having prevented him, since the publication of the former edition of his Essay on Dew, from making experiments in the open air during the night, and his reading having in the meanwhile been directed to other objects, the present edition of that Essay will be found to contain almost nothing more than the other. The chief difference between the two arises from a change in the form of several of his expressions. He has, for instance, altered the expression of 'saturation with moisture' to that of 'repletion with moisture', in order to avoid the appearance of maintaining, that common air is capable of dissolving water; a tenet unconnected with his theory. Sometimes he has subjoined to the phrase, which he now employs, on that subject, the words 'in a pellucid state;' when this addition has not been made, he wishes it to be understood.

ESSAY ON DEW, &c.

INTRODUCTION.

I WAS led, in the autumn of 1784, by the event of a rude experiment, to think it probable, that the formation of dew is attended with the production of cold. In 1788, a paper on hoarfrost, by Mr. Patrick Wilson of Glasgow, was published in the first volume of the Transactions of the Royal Society of Edinburgh, by which it appeared, that this opinion had been entertained by that gentleman, before it had occurred to myself. In the course of the same year, Mr. Six of Canterbury mentioned in a paper communicated to the Royal Society, that, on clear and dewy nights, he always found the mercury lower in a thermometer laid upon the ground, in a meadow in his neighbourhood, than it was in a similar thermometer suspended in the air, six feet above the former; and that, upon one night, the difference amounted to 5° of Fahrenheit's scale. Mr. Six, however, did not suppose, agreeably to the opinion of Mr. Wilson and myself, that the cold was occasioned by the formation of dew; but imagined, that it proceeded, partly from the low temperature of the air, through which the dew, already formed in the atmosphere, had descended, and partly from the evaporation of moisture from the ground, on which his thermometer had been placed. The conjecture of Mr. Wilson, and the observations of Mr. Six, together with many facts, which I afterwards learned in the course of reading, strengthened my opinion; but I made no attempt, before the autumn of 1811, to ascertain by experiment if it were just, though it had, in the mean time, almost daily occurred to my thoughts. Happening, in that season, to be in the country on a clear and calm night, I laid a thermometer upon grass wet with dew, and suspended a second, in the air, two feet above the other. An hour afterwards, the thermometer on the grass was found to be 8° lower, by Fahrenheit's division, than the one in the air. Similar results having been obtained from several similar experiments, made during the same autumn, I determined, in the next spring, to prosecute the subject with some degree of steadiness, and with this view went frequently to the house of one of my friends, who lives in Surrey. At the end of two months, I fancied that I had collected information worthy of being published; but fortunately, while preparing an account of it, I met, by accident, with a small posthumous work of Mr. Six, printed at Canterbury in 1794, in which are related differences observed on dewy nights, between thermometers placed upon grass and others in the air, that are much greater than those mentioned in the paper presented by him to the Royal Society in 1788. In this work, too, the cold of the grass is attributed, in agreement with the opinion of Mr. Wilson, altogether to the dew deposited upon it. The value of my own observations appearing to me now much diminished, though they embraced many points left untouched by Mr. Six, I gave up my intention of making them known. Shortly after, however, upon considering the subject more closely, I began to suspect, that Mr. Wilson, Mr. Six, and myself, had all committed an error, in regarding the cold, which accompanies dew, as an effect of the formation of that fluid. I, therefore, resumed my experiments, and having, by means of them, I think, not only established the justness of my suspicion, but ascertained the real cause both of dew, and of several other natural appearances, which have hitherto received no sufficient explanation, I venture now to submit, to the consideration of the learned, an account of some of my labours, without regard to the order of time, in which they were performed, and of various conclusions which may be drawn from them, mixed with facts and opinions already published by others.

PART I.

OF THE PHENOMENA OF DEW.

SECTION I.

Of Circumstances which influence the Production of Dew.

ARISTOTLE* and many other writers have remarked, that dew appears only on calm and serene nights. The justness of this observation, however, has not been universally admitted. For Musschenbroek † says, that dew forms in Holland, while the surface of the country is covered with a low mist; but, as he mentions at the same time, that it is deposited upon all bodies indiscriminately, the moisture, of which he speaks, cannot properly be called dew, as will be more distinctly seen hereafter. Other writers of considerable reputation have also regarded clearness of the atmosphere, as not being requisite for the production of dew, misled, I believe, partly by theory, and

* Meteor. Lib. I. c. x. et De Mundo. c. iii.
† Nat. Phil. T. ii. De Rore.

partly by observing on misty mornings copious dews, which had been produced during preceding clear nights. Respecting this point I can aver, after much experience, that I never knew dew to be abundant, except in serene weather. In regard to the necessity of the air being still, I know of no person who rejects it, except Mr. Prieur*, a late French author of little consideration, and he affirms, in opposition to the most common observation, that a fresh wind is requisite for the production of dew.

The remark of Aristotle, however, is not to be received in its strictest sense, as I have frequently found a small quantity of dew on grass, both on windy nights, if the sky was clear, or nearly so, and on cloudy nights, if there was no wind. If, indeed, the clouds were high, and the weather calm, I have sometimes seen on grass, though the sky was entirely hidden, no very inconsiderable quantity of dew. Again; according to my observation, entire stillness of the atmosphere is so far from being necessary for the formation of this fluid, that its quantity has seemed to me to be increased, by a very gentle motion in the air. Dew, however, has never been seen by me, on nights both cloudy and windy.

* Journal de l'Ecole Polytechnique, Tom. ii. 409.

If, in the course of the night, the weather, from being calm and serene, should become windy and cloudy, not only will dew cease to form, but that, which has formed, will either disappear, or diminish considerably.

In calm weather, if the sky be partially covered with clouds, more dew will appear, than if it were entirely covered, but less than if it were entirely clear.

Dew probably begins, in this country, to appear upon grass, in places shaded from the sun, during clear and calm weather, soon after the heat of the atmosphere has declined. My opportunities, however, for making such observations have not been numerous, since, while pursuing this subject, I seldom went into the country, till late in the afternoon; but I have frequently felt grass moist, in dry weather, several hours before sunset. On the other hand, I have scarcely ever known dew to be present in such quantity upon grass, as to exhibit visible drops, before the sun was very near the horizon, or to be very copious, till some time after sunset. It also continues to form, in shaded places, after sunrise; but the interval between sunrise, and its ceasing to form, is, according to my observation, which, upon this point, has not been extensive, considerably shorter, than that between its first appearance in the afternoon, and sunset. Contrary, however, to what happens at sunset, if the weather be favourable, more dew forms a little before, and, in shaded places, a little after sunrise, than at any other time. Musschenbroek, therefore, errs greatly when he says, that dew does not form after the sun has risen. The preceding observations, on the early appearance of dew in the afternoon, are to be restricted to what happens to grass, or other substances highly attractive of dew placed on the ground; for it occurs much later on similar substances, which are elevated a few feet above the ground, though upon these it continues to form, as long after the rising of the sun, as upon the others, if they be equally sheltered from the rays of that body.

The formation of dew, after it has once commenced, continues during the whole night, if the weather remain still and serene. Mr. Prieur, indeed, of whom I have already spoken, asserts, that dew forms only in the evening and morning, and that any which occurs in the former season always disappears in the course of the night. I can affirm, however, from long experience, that grass, after having been dewed in the evening, is never found dry until after sunrise, unless the weather has, in the mean time, changed. Upon one serene and still night, I placed fresh parcels of wool upon grass every hour, and by weighing each of them, after exposure for an hour, found, that they had all attracted dew.

When dew forms upon a smooth dense body as glass, and it is only by means of such a body, that the process can be accurately observed, the appearances are altogether similar to those, which occur on a like body, when exposed to the steam of water, a little warmer than itself. The exposed surface has first its lustre diminished, by a slight damp uniformly spread over it. As the moisture increases, it gathers into irregularly shaped flat drops, which are, at first, very small, but afterwards enlarge and run into one another, forming streamlets, by means of which a great part escapes from the body which had received it.

During nights, that are equally clear and calm, dew often appears in very unequal quantities, even after allowance has been made, for any difference in their lengths. One great source of these differences is very obvious. For, it being manifest, whatever theory be adopted concerning the immediate cause of dew, that the more replete the atmosphere is with moisture, previously to the operation of that cause, the more copious will the precipitation of water be, after this operation has commenced, all the circumstances, which tend to increase the quantity of moisture in the atmosphere, must likewise tend to increase the production of dew. Thus dew, in equally calm and clear nights, is more abundant shortly after rain, than during a long tract of dry weather. It is more abundant, also, throughout Europe, with perhaps a few exceptions, and in some parts of Asia and Africa, during southerly and westerly winds, than during those, which blow from the north and the east. Aristotle* says, that Pontus is the only country, in which dew is more copious during a northerly, than during a southerly wind. But a similar fact occurs in Egypt; for dew is scarcely ever observed there, except while the Etesian winds prevail. Both cases, however, though contrary to the letter, are consonant with the spirit of the rule; since the north wind, in one country, proceeds from the Euxine sea, and, in the other, from the Mediterranean. Another circumstance, of the same kind with the blowing of wind from the south and the west, as shewing that the air contains much moisture, is the lessening of the weight of the atmosphere. My experience on this point has not, indeed, been great, as the falling of the mercury in the barometer is very commonly attended with wind or clouds, both unfavourable

* Meteor. Lib. 1. c. x.

to the production of dew; but still the greatest dew, I have ever witnessed, occurred while the barometer was sinking. A corresponding observation is made by Mr. de Luc, who says, that rain may be foretold, when dew is uncommonly abundant, in relation to the climate and season*.

To the greater or less quantity of moisture in the atmosphere, at the time of the action of the immediate cause of dew, are likewise to be referred several other facts respecting its copiousness, the explanation of which is, perhaps, not so apparent, as in the preceding examples.

In the first place; dew is commonly more plentiful in spring and autumn, than in summer; the reason is, that a greater difference is generally found between the temperatures of the day and the night, in the former seasons of the year, than in the latter. In spring, this circumstance is prevented often from having a considerable effect, by the opposite influence of northerly and easterly winds; but, during still and serene nights in autumn, dew is almost always highly abundant.

In the second place; dew is always very copious, on those clear and calm nights, which are followed by misty or foggy mornings; the

* Rech. sur les Mod. de l'Atm. § 725.

turbidness of the air in the morning shewing, that it must have contained, during the preceding night, a considerable quantity of moisture.

Thirdly; I have observed dew to be unusually plentiful on a clear morning, which had succeeded a cloudy night. For the air, having in the course of the night lost little or no moisture, was in the morning more charged with watery vapour, than it would have been, if the night had also been clear.

Fourthly; heat of the atmosphere, if other circumstances are favourable, which, according to my experience, they seldom are in this country, occasions a great formation of dew. For, as the power of the air, to retain watery vapour in a pellucid state, increases considerably faster, while its temperature is rising, than in proportion to the heat acquired, a decrease of its heat, in any small given quantity, during the night, must bring it, if the temperature be high, much nearer to the point of repletion, before it be acted upon by the immediate cause of dew, than if the temperature were low. We read, accordingly, in the writings of those, who have travelled into hot climates, of a copiousness of dew frequently observed by them there, which very much exceeds what occurs, at any time, in this country. But even here, dew, though for the most part scanty in our hottest season, is sometimes very abundant during it, an example of which occurred to me on the night, common to the 29th and 30th of July 1813; for on that night, notwithstanding its shortness, more dew appeared, than has ever been observed by me on any other.

In the last place; I always found, when the clearness and stillness of the atmosphere were the same, that more dew was formed between midnight and sunrise, than between sunset and midnight, though the positive quantity of moisture in the air, must have been less in the former, than in the latter time, in consequence of a previous precipitation of part of it. The reason, no doubt, is the cold of the atmosphere being greater in the latter, than in the prior part of the night.

But there are many circumstances, influencing the quantity of dew, which, though much more open to accurate observation, than those hitherto mentioned, are yet much less easy to be understood.

In my first attempts to compare the quantities of dew formed during different times, or in different situations, I attended only to the appearance, which it made on bodies having smooth surfaces. But quickly seeing this method to be very imperfect, I next employed wool to collect dew from the atmosphere, and found it well adapted for my purpose, as it readily admits amongst its fibres the moisture, which forms on its outer parts, and retains what it receives so firmly, that I never but once had occasion to suspect, that it suffered any portion of what it had thus acquired to pass entirely through it. The wool, which I used, was white, moderately fine, and already imbued with a little moisture, from having been long exposed to the air of a room, in which no fire was kept. I divided it into parcels of 10 grains each, and, immediately before exposure, pulled the fibres of every parcel somewhat asunder, so as to give it the form of a flattened sphere, the greatest diameter of which was about 2 inches. As in doing this, I went by the judgment of my sight alone, some little inequality, in point of size, must have existed among different parcels, but none, I think, sufficient to affect the accuracy of my conclusions from the experiments, in which they were employed, more especially as my conclusions scarcely ever rested upon single trials.

Previously to mentioning the results of any of my experiments with these parcels of wool, I think it right to describe the place, where by far the greater part of my observations on dew were made. This was a garden in Surrey, distant, by the public road, about three miles from the bridge over the Thames at Blackfriars, but not more than a mile and a quarter, from a densely built part of the suburbs on the south side of that river. The form of the garden was oblong, its extent nearly half an acre, and its surface level. At one end was a dwelling-house of moderate size, at the other a range of low buildings; on one side a row of high trees, on the other a low fence, dividing it from another garden. If this fence had been absent, the garden would have been on the latter side entirely open. Within it were some fruit trees, but, as it had not been long made, their size was small. Towards one end, there was a grass-plat, in length 62 feet, and nearly 16 broad, the herbage of which was kept short by frequent mowing. The rest of the garden was employed for the production of culinary vegetables. All of these circumstances, however trifling they may appear, had influence on my experiments, and most of them, as will hereafter be seen, must have rendered the results less remarkable, than they would have been, if they had occurred on a wide open plain, considerably distant from a large city.

I now proceed to relate the influence, which several differences in the situation, mechanical state, and real nature of bodies, have upon the production of dew.

I. One general fact relative to situation is, that whatever diminishes the view of the sky, as seen from the exposed body, occasions the quantity of dew, which is formed upon it, to be less than would have occurred, if the exposure to the sky had been complete.

I placed, on several clear and still nights, 10 grains of wool upon the middle of a painted board, 4½ feet long, 2 feet wide, and 1 inch thick, elevated 4 feet above the grassplat, by means of 4 slender wooden props of equal height; and, at the same time, attached, loosely, 10 grains of wool to the middle of its underside. The two parcels were consequently only an inch asunder, and were equally exposed to the action of the air. Upon one night, however, I found, that the upper parcel had gained 14 grains in weight, but the lower only 4. On a second night, the quantities of moisture, acquired by like parcels of wool, in the same situations as in the first experiment, were 19 and 6 grains; on a third, 11 and 2; on a fourth, 20 and 4; the smaller quantity being always that, which was gained by the wool attached to the lower side of the board.

I bent a sheet of pasteboard into the shape of a house-roof, making the angle of flexure 90 degrees, and leaving both ends open. This was placed one evening, with its ridge uppermost, upon the same grassplat, in the direction of the wind, as well as this could be ascertained. I then laid 10 grains of wool on the middle of that part of the grass, which was sheltered by the roof, and the same quantity on another part of the grassplat fully exposed to the sky. In the morning, the sheltered wool was found to have increased in weight only 2 grains, but that, which had been exposed to the sky, 16 grains.

In these experiments, the view of the sky was almost entirely cut off from the situations, in which little dew was formed. In others, where it was less so, the quantity gained was greater. Thus, 10 grains of wool, placed upon the spot of the grassplat, which was directly under the middle of the raised board, and which enjoyed, therefore, a considerable oblique view of the sky, acquired during one night 7, during a second 9, and during a third 12 grains of moisture, while the quantities gained, during the same times, by equal parcels of wool, laid upon another part of the grassplat, which was entirely exposed to the heavens, were 10, 16, and 20 grains.

As no moisture, falling like rain from the atmosphere, could, on a calm night, have reached the wool in any of the situations, where little dew was formed, it may be thought, that the substances, under which the wool was placed, prevented, mechanically, the access of that fluid. But on this supposition it cannot be explained, why some dew was always found in the most sheltered places, and why a considerable quantity occurred upon the grass under the middle of the raised board. A still stronger proof of the want of justness in this supposition is afforded by the following experiment. I placed, upright, on the grassplat a hollow cylinder of baked clay, the height of which was 2½ feet, and diameter 1 foot. On the grass, surrounded by the cylinder, were laid 10 grains of wool, which, in this situation, as there was not the least wind, would have received as much rain, as a like quantity of wool fully exposed to the sky. But the quantity of moisture, obtained by the wool surrounded by the cylinder, was only a little more than 2 grains, while that acquired by 10 grains of fully exposed wool was 16. This occurred on the night, during which the wool under the bent pasteboard gained only 2 grains of moisture.

Dew, however, will, in consequence of other varieties of situation, form in very different quantities, upon substances of the same kind, although these should be similarly exposed to the sky.

In the first place; it is requisite, for the most abundant formation of dew, that the substance attracting it should rest on a stable horizontal body of some extent. Thus, upon one night, while 10 grains of wool, laid upon the raised board, increased 20 grains in weight, an equal quantity, suspended in the open air, $5\frac{1}{2}$ feet above the ground, increased only 11 grains, notwithstanding that it presented a greater surface to the air than the other parcel. On another night, 10 grains of wool gained on the raised board 19 grains, but the same quantity suspended in the air, on a level with the board, only 13; and on a third, 10 grains of wool acquired, on the same board, $2\frac{1}{2}$ grains of weight, during the time in which other 10 grains, hung in the air, at the same height, acquired only $\frac{1}{2}$ a grain.

In the second place; the quantities of dew attracted by equal masses of wool, similarly exposed to the sky, and resting on equally stable and extended bodies, oftentimes vary considerably, in consequence of some difference in the other circumstances of these bodies. 10 grains of wool, for instance, having been placed upon the grassplat, on a dewy evening, 10 grains upon a gravel walk which bounded the grassplat, and 10 grains upon a bed of bare garden mould, immediately adjoining the gravel walk; in the morning, the wool on the grass was found to have increased 16 grains in weight, but that on the gravel walk only 9, and that on the garden mould only 8. On another night, during the time that 10 grains of wool, laid upon grass, acquired $2\frac{1}{2}$ grains of moisture, the same quantity gained only $\frac{1}{2}$ a grain upon the bed of garden mould, and a like quantity, placed upon the gravel walk, received no accession of weight whatever.

Two objections will probably be made against the accuracy of these, as well as my other experiments with wool. One is, that wool placed on grass may, by a kind of capillary attraction, receive dew previously formed on the grass, in addition to its own. To this I answer, that wool in a china saucer, placed on the grass, acquired very nearly as much weight, as an equal parcel immediately touching the grass. The second objection is, that a part of the increased weight in the wool might arise from its imbibing moisture, as a hygroscopic substance. I do not deny, that some weight was given to the wool in this way; but it may be safely affirmed, that this quantity must have been very small. For, on very cloudy nights, apparently the best fitted to increase the weight of hygroscopic substances, wool upon the raised board would, in the course of many hours, acquire little or no weight; and in London, I have never found

10 grains of wool, exposed to the air on the outside of one of my chamber windows, to increase, during a whole night, more than $\frac{1}{2}$ a grain in weight. When this weight was gained, the weather was clear and still; if the weather was cloudy and windy, the wool received either less or no weight. This window is so situated, as to be, in great measure, deprived of the aspect of the sky.

It being shewn, that wool, though highly attractive of dew, was prevented, by the mere vicinity of a gravel walk, or a bed of garden mould, for only a small part of it actually touched those bodies, from acquiring nearly as much dew, as an equal parcel laid upon grass, it may be readily inferred, that little was formed upon themselves. In confirmation of this conclusion, I shall mention, that I never saw dew upon either of them. Another fact of the same kind is, that, while returning to London from the scene of my experiments about sunrise, I never observed, if the atmosphere was clear, the public road, or any stone pavement on the side of it, to be moistened with dew, though grass within a few feet of it, and painted doors and windows of houses not far from it, were frequently very wet. If, indeed, there was a foggy morning, after a clear and calm night, even the streets of London would sometimes be moist, though they had been dry the day before, and no rain had in the meanwhile fallen. This entire, or almost entire, freedom of certain situations from dew depends, however, much more upon extraneous circumstances, than upon the nature of the substances found there; for river sand, though of the same nature with gravel, when placed upon the raised board, or upon grass, attracted dew copiously.

A third difference, from situation, in the quantity of dew collected by similar bodies, similarly exposed to the sky, depends upon their position with respect to the ground. Thus, a substance placed several feet above the ground, though in this situation later dewed, than if it touched the earth, would, notwithstanding, if it lay upon a stable body of some extent, such as the raised board lately mentioned, acquire more dew during a very still night, than a similar substance lying on grass.

A fourth difference of this kind occurred among bodies placed on different parts of the raised board. For one, that was placed at the leeward end of it, generally acquired more dew than a similar body at the windward extremity.

II. Difference in the mechanical state of bodies, though all other circumstances be similar, has likewise an effect on the quantity of dew, which they attract. Thus, more dew is formed upon fine shavings of wood, than upon a thick piece of the same substance. It is chiefly for a similar reason, I believe, that fine raw silk, fine unwrought cotton, and flax, were found by me to attract somewhat more dew, than the wool I employed, the fibres of which were thicker, than those of the other substances just mentioned.

III. Bright metals, in consequence of some circumstance in their constitution, attract dew much less powerfully than other bodies; all of which, after allowance has been made for any difference, which may exist in their mechanical states, seem to attract dew in quantities not very unequal, if they be similarly situated.

Musschenbroek was the first, who distinctly remarked this peculiarity of metals; but Dufay*, I believe, published it before him, referring, at the same time, the discovery to its proper author. Both Musschenbroek and Dufay, however, made too large an inference from their experiments; for they asserted, that dew never appears on the upper surface of bright metals, whereas the contrary has since been observed by many persons, and I have myself known dew to form on gold, silver, copper, tin, platina, iron, steel, zinc, and lead. Dew, however,

* Mem. de l'Acad. Fran. 1736.

when it does form upon metals, commonly sullies only the lustre of their surface; and even when it is sufficiently abundant to gather into drops, these are almost always small and distinct. Two other facts of the same kind are; *first*, that the dew, which has formed upon a metal, will often disappear, while other substances in their neighbourhood remain wet; and *secondly*, that a metal, which has been purposely moistened, will often become dry, though similarly exposed with bodies which are attracting dew. This inaptitude to attract dew, in metals, is communicated to bodies of a very different nature, which touch or are near to them. For I have found, that wool laid upon a metal will acquire much less dew, than an equal quantity laid upon grass in the immediate vicinity.

A large metallic plate, lying on grass, resists the formation of dew more powerfully than a very small one similarly situated. I conclude from various collateral facts, that a considerable difference in the thickness of two pieces of metal, exposing equal surfaces to the sky, will be attended with a similar consequence, wherever they be placed, though I have no observation, which proves this directly. If, however, a large and a very small plate be suspended horizontally, at the same height, in the air, the small plate will resist the formation of dew more powerfully than the large.

If a metal be closely attached to a substance of some thickness, which attracts dew powerfully, the attraction of the metal itself for dew, instead of being increased from this circumstance, becomes diminished, provided the metal cover the whole of the upper surface of the other body. If only a part of this body be covered, the production of dew on the metal is forwarded by the conjunction, and this somewhat in proportion, to the quantity of surface in the lower body left uncovered. The justness of the first of these observations is proved by the following experiment. I joined, in the form of a cross, two pieces of very light wood, each 4 inches long, a third of an inch in breadth, and 1 line in thickness. To one side of this cross I fastened, by means of mucilage, a square piece of gilt paper, and then exposed the instrument to the sky, with its metallic side uppermost, on a dewy night, by suspending it, in a horizontal position, about 6 inches above the ground. A few hours after, the unattached parts of the metalled paper were found covered with minute drops of dew, while those, which adhered to the cross, were dry.

A large metallic plate, laid upon grass, was dewed with more difficulty on its upper surface, than a similar plate elevated a few inches above the grass, by means of slender props, which allowed the air to pass freely under the metal. But the case with respect to small pieces was the reverse; for I have often seen, covered with dew, the metallic sheath of a small thermometer lying upon grass, while the similar sheath of another thermometer, suspended in the air, remained dry.

Removing a metal several times, in the course of the night, from one part of the grassplat to another, facilitated its being dewed. The same effect was produced on gilt and silvered paper, by first exposing them to the sky, for some time, with the bare side uppermost, and then turning them.

If a piece of glass, covered on one side with a metal, be placed upon the ground, with this side downwards, the upper surface will attract dew, precisely as if no metal were attached to the lower surface.

The upper surfaces of metals are most readily, and most copiously dewed, on those nights, and in those parts of the night, during which other substances are the most readily, and the most copiously dewed.

If a metallic plate had been laid upon grass, before dew began to form anywhere, its lower side, notwithstanding, always became moist in the course of the night; and the same effect was almost always observed, if the plate had been placed horizontally in the air, a few inches above the grass. While the undersides were thus moist, the upper surfaces were very often dry. If, however, the plate was elevated several feet in the air, the condition of both sides was always the same, whether this was dry or moist.

The remarks hitherto made, on the relation of metals to dew, apply to the class generally; but it is now to be mentioned, that they do not all resist the formation of that fluid, with the same force.

I saw, for example, platina one night distinctly dewed, while gold, silver, copper and tin, though similarly situated, were entirely dry; and I have also several times seen these four metals free from dew, while iron, steel, zinc, and lead were covered with it.

I once supposed, in consequence of the difficulty with which metals are dewed, that they might in all circumstances resist, in a greater degree than other bodies, the condensation of watery vapour upon their surface; and I afterwards found, that Le Roi* asserts this to be the case. But having exposed at the same time, to the steam of warm water, pieces of glass and

* Mem. de l'Acad. Fran. 1751.

of metal, I did not see, that moisture formed in the least more readily, upon the former than upon the latter. I have since learned, that Saussure* once entertained a similar suspicion, which was also proved by an experiment to be groundless.

All my experiments, hitherto spoken of, were made in the country. But Le Roi having said, that dew is never deposited by the air of cities, I determined to ascertain, if his assertion was just. With this view, I frequently exposed, at night, 10 grains of wool upon a slight wooden frame, placed in such a manner, between two ridges of the top of my house, which is situated in one of the most crowded districts of London, as to be 3 feet distant from the nearest part of the roof. The event was, that, upon clear and calm nights, dew was always acquired by the wool, though never in any considerable quantity; probably, however, more from the wooden frame being nearly surrounded by buildings, much more elevated than itself, than from any particular condition of the air in cities. The formation of dew, in this situation, proceeded much less regularly than in the country. For,

* Hygronometrie, page 329.

upon one evening, 10 grains of wool gained in it 3 grains of moisture, in 1 hour and 18 minutes, though I scarcely ever knew a greater quantity to be collected by a similar parcel of wool, in the same place, during a whole night. These experiments will no doubt seem to many superfluous, since dew may be observed every fine evening, upon grass in London. But as dew upon grass is said by Le Roi to proceed from the ground, and not from the atmosphere, the argument derived from its appearance there, in cities, against his assertion is thus eluded by him.

The last subject, which I shall here touch upon, is that of hoarfrost.

This substance has, I believe, from the time of Aristotle*, been uniformly, and, according to my observations, justly, considered as frozen dew. I shall, therefore, frequently refer hereafter to the experiments of the late Mr. Patrick Wilson of Glasgow respecting it, as if they had been actually made upon that fluid. Indeed, several of my experiments upon dew were only imitations of some, which had been previously made upon hoarfrost, by that ingenious and most worthy man.

* Meteor. Lib. I. c. x.

SECTION II.

Of the Cold connected with the Formation of Dew.

Dew is often spoken of as being cold, by popular writers. Thus Cicero and Virgil apply to it the epithet of 'gelidus,' Milton that of 'chill,' and Collins that of 'cold.' Of the same import is a passage in Herodotus, in which it is said, that, in Egypt, the crocodile passes a great part of the day on dry land, but the whole of the night in the Nile, this being warmer than the atmosphere, and the *dew*. Among philosophers, however, Mr. Wilson was the first, I believe, who ever suspected the existence of such a conjunction.

In my experiments on the temperature of bodies moistened with dew, small thermometers were employed, (the largest being only 8 inches long) having globular bulbs, which, in most of them, were not more than from 2 to $2\frac{1}{2}$ lines in diameter. Their scales, which were marked in the manner of Fahrenheit, were of ivory or wood, and were furnished, almost all of them, with hinges. They were always employed naked, except I wished to know the effect of covering them with any particular substance.

By means of these instruments I have very many times, during serene and still nights, examined the temperature of dewed grass, and have constantly observed it to be less than that of the air, anywhere between 1 inch and 9 feet above the ground, the latter being the greatest height, at which I ever marked the heat of the atmosphere, in these experiments. I generally, however, compared the temperature of dewed grass with that of the air 4 feet above the ground; and on nights, that were calm and clear, very frequently found the grass, at the ordinary place of my observations, 7, 8, or 9 degrees colder than the air at that height. Several times it was 10° and 11° colder than the air, and once 12°. These differences are not so great, as those related in Mr. Six's posthumous work. But, in his experiments, the temperature of grass was compared with that of the air 7 feet above the ground, which, in clear and calm nights, may be regarded as ½ a degree warmer than the air at the height of 4 feet. Besides; the most considerable differences, mentioned by Mr. Six, occurred in winter, when he says a greater degree of cold is occasioned by dew, than at any other time; whereas very few of my experiments, on the temperature of grass, were instituted in that season. In the last place; my experiments were almost always made on very short grass, while Mr. Six's thermometers were laid upon long grass bent, by strong pressure, towards the earth; in which state they marked a temperature 1, 2, and 3 degrees lower, than that shewn by similar thermometers placed upon grass, less than an inch in height. Had it not been for these circumstances, and the unfitness, in various respects, besides the shortness of the grass, for the production of a great cold, of the common scene of my operations, I believe that, in consequence of my thermometers being much better adapted to mark a superficial, or transitory cold, than those of Mr. Six, I should at some time have seen a difference several degrees greater, than the greatest ever seen by that gentleman, which was one of $13\frac{1}{2}°$. In confirmation of this opinion, I shall mention, that having, during a short visit to a more distant part of the country, exposed, in the evening, a thermometer upon the surface of an open grass field, I found it soon after, although the grass was short, and the weather warm, 14° lower than a similar thermometer, suspended in the air, 4 feet above the grass. If to this quantity be added $\frac{1}{2}$ a degree, on account of the difference in elevation between our suspended thermometers, the cold, connected with dew, observed by me this night on grass, will exceed the greatest ever observed by Mr. Six by 1 degree.

According to a few observations made by me, the greater coldness of grass, than that of the air, begins to appear, in clear and calm weather, in places, sheltered in the afternoon from the sun, but still open to a considerable portion of the sky, soon after the heat of the atmosphere has declined. A similar coldness continues upon grass in still and serene mornings, for some time after the rising of the sun, in places shaded from its direct light, but otherwise open to the sky. My experiments on this point have also not been many, and none of them were made in winter; which, I presume, are the reasons, that I never observed a cold, from this cause, later in the morning, than an hour after sunrise. The surface of snow, however, was once, in the depth of winter, observed by Mr. Wilson of Glasgow to be considerably colder than the air, till a little after midday*.

In cloudy nights, particularly if there was wind, the grass was never much colder than the air. On such nights, the temperatures of both were sometimes the same; at other times that of the grass was the higher of the two,

* Paper in Phil. Trans. 1781.

even when the grass was wet from preceding rain, and when, consequently, it must have been, in some measure, cooled by evaporation. On one such night, the grass was found to be 4° colder than the earth an inch beneath the surface of the plat, which afforded a sufficient reason for the grass itself being warmer than the air. In windy weather, however, if the sky was clear, some degree of cold, in addition to that of the air, was always observed upon the grass; and in calm weather, very high clouds, though sufficiently extensive and dense, to conceal the sky completely, would yet frequently allow of the grass being several degrees colder than the air. I once observed, upon a night of this kind, a difference of 5° between the temperatures of those bodies.

If the night became cloudy, after having been very clear, though there might be no change with respect to calmness, a considerable alteration in the temperature of the grass always ensued; and this sometimes very suddenly. Upon one such night, the grass, after having been 12° colder than the air, became only 2° colder than it, the temperature of the air being the same at both observations. On a second night, grass became 9° warmer in the space of an hour and a half. On a third night, in less than 45 minutes, for the whole change occurred while I was absent 45 minutes, the temperature of the grass rose 15°, while that of the neighbouring air increased 3½°. During a fourth night, the temperature of the grass at half-past 9 o'clock was 32°. In 20 minutes afterwards, it was found to be 39°, the sky having in the mean time become cloudy. At the end of 20 minutes more, the sky being clear, the temperature of the grass was again 32°. These were the most remarkable of my observations on this subject; but I may add to them, that I have frequently seen, during nights that were generally clear, a thermometer lying on the grass-plat rise several degrees, upon the zenith being occupied only a few minutes by a cloud. On the other hand, upon two nights I observed a very great degree of cold to occur on the ground, in addition to that of the atmosphere, during short intervals of clearness of sky, between very cloudy states of it.

I did not speak in the preceding section of another obscure state of the atmosphere, that occasioned by fog, or mist, as the moisture deposited in it attaches to all bodies, indiscriminately; on which account, I was unable to determine, whether or not dew forms during its continuance. But, with respect to the connexion of this condition of the atmosphere with cold, I have to remark, that I have several times, on its appearance betwixt daybreak and sunrise, found the difference between thermometers on grass and in the air, which had been considerable during the night, to diminish greatly. I never, indeed, observed it to vanish, but this I used to impute to the air being not very much obscured. I have now, however, reason to doubt the justness of this conclusion; for on the evening of the 1st of January in the present year, 1814, I found, during a dense fog, while the weather was very calm, a thermometer lying on grass, thickly covered with hoarfrost, 9° lower than another suspended in the air, 4 feet above the former. On the following evening, when the air was equally calm, but the fog sufficiently attenuated to allow me to see that the sky was almost entirely covered with clouds, the difference between two thermometers, similarly placed with the former, was only 1°. On comparing the observations of these two evenings, I conclude, that on the first few or no clouds existed above the fog, and consequently that fog, if there be no clouds above it, may, in a very calm air, admit of the appearance of a considerable degree of cold, at night, upon the surface of the earth, in addition to that of the atmosphere. Mr. Six, indeed, says, while speaking of the cold connected with dew, in his paper in the Philosophical Transactions for 1788, "fogs did not, as far as I could perceive, at all impede, but rather increase, the refrigeration." But this was a mistake; which in all probability arose from his ascribing the effect of a clear night to an ensuing foggy morning, as he examined his thermometers only in the daytime. He afterwards discovered his error; for, in his posthumous work, thick fogs are ranked among the circumstances, which always impede, and sometimes prevent altogether, the appearance of a cold upon the surface of the earth, greater than that of the atmosphere. During a very dense fog, Mr. Wilson found no difference, at night, between a thermometer laid upon snow, and another suspended in the air *.

When, during a clear and still night, different thermometers were examined, at the same time, which had been placed in different situations, those which were situated, where most dew was formed, were always found to be the lowest. Thus, upon one such night, I found a thermometer placed upon a little wool, lying upon the middle of the upper side of the raised board, to be 9° lower than another thermometer, in contact with an equal quantity of wool, attached to the middle of the underside of the board. On

* Edin. Phil. Trans. I. 170.

two other nights, the difference between two thermometers in the same situations was 8°. I found also, on two other serene and calm nights, a spot of grass covered by the pasteboard roof, and another spot surrounded by the earthen cylinder, to be both 10° warmer than neighbouring grass fully exposed to the sky. Thinking it possible, that the cylinder, which had been exposed to the sun the preceding day, might still possess some of the heat, which it had then imbibed, I placed near to it, on another night, a cylinder made of very thin pasteboard; but this was equally efficacious with the earthen one, in preventing cold from occurring on grass. When the exposure was greater than in the preceding examples, and more dew was in consequence formed, the cold was also greater, but still less than where the exposure was complete. For instance, upon the night during which 10 grains of wool, placed upon the middle of the grass, which was sheltered by the raised board, had gained 7 grains, and the same quantity on grass fully exposed to the sky had gained 10 grains, the difference between the temperatures of the two portions of grass was only 2½°.

The same correspondence was observed, when the differences in the quantity of dew did not depend, as in the preceding instances, upon any diversity of exposure to the sky. Thus, the mercury in a thermometer placed upon wool, lying on the raised board, was found to be at the 44th degree, while that in another, pendent in the air, at the same height from the ground, and wrapped in wool, was at the 48th. Wool also, on the raised board*, was commonly a little colder than the same substance on grass, when the night was very still; and the leeward end of that board was generally colder than the windward extremity.

But, the most remarkable examples of this kind were exhibited by the gravel walk, and the bare garden mould. In still and serene nights, the surfaces of these bodies were always warmer than the neighbouring grass, and frequently warmer than the air. On one night of this description, I observed, $2\frac{1}{2}$ hours after sunset, the surface of the gravel walk to be $16\frac{1}{2}°$, and that of the garden mould to be $12\frac{1}{2}°$, warmer

* The greater cold of the raised board, in my experiments, most probably depended on the grass being very short; since Mr. Wilson found, that snow on the ground was colder than the same body on a raised board. If 1, 2, or 3 degrees were added to the cold of the grass at my place of observation, agreeably to the difference found by Mr. Six, between the temperatures of long and short grass in dewy nights, the cold on my raised board would, upon such nights, have been always less than that of the grassplat.

than grass very near to them, and similarly exposed to the heavens. As the night proceeded, clouds formed and accumulated; in consequence of which the difference at sunrise, between the temperatures of the grass and the gravel walk, was only 6°, and between those of the grass and the mould only 4°, the temperature of the grass having in the mean time increased considerably, while that of the other bodies had decreased a little. At another time, shortly before sunrise, a very clear morning having succeeded a cloudy night, I found the gravel walk to be 10° and the garden bed to be 9° warmer than neighbouring grass, which was 8° colder than the air. Both of these examples occurred in summer, and I believe, that such considerable differences will occur in that season only. It was on the first of these two nights, that 10 grains of wool gained only $\frac{1}{2}$ a grain of moisture on the mould, and that the same quantity gained no weight on the gravel walk. That the unfitness of the gravel walk, however, to become cold, like its unfitness to attract dew, arose from its situation, and not from the nature of the substance of which it was made, is proved by this circumstance, that river sand, placed on the raised board, was on 4 different nights, none of them highly favourable for the production of cold, 7, 7, 8, and $8\frac{1}{2}$ degrees colder than the air at the same height.

It may be added here, that I have always found, on dewy nights, the temperature of the earth, $\frac{1}{2}$ an inch or an inch beneath its surface, much warmer than the grass upon it. On five such nights the differences were from 12 to 16 degrees. The earth, at the above-mentioned depth, was also almost constantly warmer on dewy nights than the air; sometimes it was considerably so, for I once observed it to be 10° warmer, at another time 9°, and at a third $7\frac{1}{2}$°. An exception will no doubt occur, if very mild weather should follow a long frost; but of this I have had no experience.

In the experiments upon my housetop in London, I always found, during clear and calm nights, wool lying on the wooden frame to be colder than the air, at the same height; but the difference was seldom more than 3°. On the evening, however, during which dew formed there more copiously than usual, the difference was 5°. That the smallness of these differences was not wholly occasioned by any thing special in the air of cities was afterwards proved, by my finding others much greater, in a garden nearly in the middle of London, from which almost the whole of the sky was visible.

Metals, likewise, furnish proofs of the connexion of dew with a cold in the substance, on which it forms, superior to that of the neighbouring atmosphere. My observations, however, on the temperature of metals, when exposed to the sky on dewy nights, were less numerous, than those on several other subjects treated in this Essay, by reason of the less frequent opportunity I enjoyed of making them; and many of those, which I did make, were afterwards found by me to have been improperly conducted. I thought, for instance, for some time, that the temperature of a metal, on a dewy night, might easily be learned in the way, in which I had been accustomed to ascertain the temperature of dewed grass. But, observing dew one night on the glass tube of a thermometer, which was lying on a metal placed upon grass, while the metal itself was free from moisture, I conceived it probable, that the cold then indicated by the thermometer was not the real temperature of the body, to which it was applied. To determine the point, I placed on the same metal a second thermometer, covered with gilt paper, upon which this was found at three observations to be $6\frac{1}{2}°$, $7°$, and $7°$ higher than the other. In this experiment, the bulb of the naked thermometer, from being very small, did not project as far as the outer surface of the scale, and, consequently, did not come in contact with the metal. But even when the ball of a thermometer was applied directly to a metal, on a clear and calm night, a temperature was marked by it, commonly 2 and 3, and sometimes more degrees less than that marked by a similar thermometer, inclosed in gilt paper, and similarly placed. I found it likewise necessary, in this inquiry, to correct the temperature of the air, as given by a naked thermometer. For, on still and serene nights, a thermometer inclosed in a case of gilt or silvered paper, and suspended in the air 4 feet above the grassplat, was usually observed to be $1\frac{1}{2}°$ or $2°$ higher than a bare thermometer, of the same construction, suspended near to it. The difference of two such thermometers, thus placed, was once observed by me to be $2\frac{1}{2}°$, and once $3\frac{1}{2}°$. It may be thought, perhaps, that these differences were caused by the metalled case obstructing the transmission of the temperature of the air to the inclosed instrument. But that this was not the reason is shewn by my observing, that on cloudy nights there existed no difference between the two thermometers; that, even on clear nights, a thermometer contained in a case of white paper, somewhat thicker than the metalled, was always nearly of the same temperature with a naked one which was suspended close to it; and that, when a difference did exist between the two latter, the thermometer in the white paper case was commonly lower than the other.

The estimation of the heat, both of air and of metals, on a dewy night, is liable to errors from other causes. As these, however, are trifling, I shall not mention them, but proceed to state the results of my observations, upon the temperature of metals exposed to the sky at night, though unable to vouch for their entire accuracy.

Thin bright metallic plates, the least having a surface of 25 square inches, and some of them a surface of more that 100 such inches, were several times observed, while lying on grass which was attracting dew, to be 1 and 2, and once 3, degrees warmer than the air 4 feet above them. At other times, their temperature was the same with that of the air. In both of these cases their upper surfaces were always free from dew. Metals thus situated were, consequently, often much warmer than the grass, which surrounded them. I made no experiments on this point, during the nights, on which occurred the greatest instances of cold on grass, relatively to the temperature of the air; but I found, notwithstanding, during one night, a metal on grass to be 10° warmer than the exposed grass near to it. On two other nights, the differences were 9° and 8°. The superiority of the heat of metals on grass over that of the air, when it did exist, was evidently connected with the temperature of the grass, which they covered, and this again with that of the earth under the same portion of grass; for this portion was always a little warmer than the metal, but not so warm as the earth.

On the other hand, metals, on which dew was forming while they lay upon grass, were always colder than the air. In like manner, if one metal upon the grassplat were dewed, while another similarly situated remained dry, the former was always colder than the latter.

When a metal lying on the grassplat became dewed, the grass under it was always colder than that under another metal, which was undewed.

A metal, while receiving dew, in consequence of being elevated in the air, was always colder than a similar metal, which remained undewed on the grass.

The greatest instances of cold, observed by me on metals, occurred at times, when other bodies near to them had become considerably colder than the atmosphere.

The cold, however, contracted by metals, from exposure to the sky in a clear and still night, was always less than that of other bodies similarly situated, the greatest excess of cold ever observed by me, in the larger metallic plates, from this cause, over that of the air, being not more than 3 or 4 degrees. If much smaller pieces were placed upon grass, the result was different. For I have found a small thermometer placed in this situation, while inclosed in a sheath of gilt paper, to be only 3° less cold than the surrounding grass, during a night favourable to the production of cold on the surface of the earth.

I collected only a few facts respecting the comparative temperatures of different metals, when they were exposed together to the sky, on dewy nights; but such as I did collect tend to prove, that the most readily dewed metals become colder than the air, sooner than those, which receive dew with greater difficulty.

Many of the experiments, which have been mentioned in this section, shew, that when bodies, which had been equally exposed to the night air, were examined at the same time, those which were most dewed were also the coldest. No such correspondence, however, was found in the experiments of different nights, or even of different parts of the same night. Thus, during two nights, on which grass was 12° and 14° colder than the air, there was little dew; while on the night, which afforded the most copious dew ever observed by me, the cold possessed by the grass, beyond that of the air, was for the most part only 3° and 4°; and I have always seen less dew about sunset, than about sunrise, when the weather has been calm and clear at both times, though there is commonly, in this country at least, a greater difference between the temperature of grass and of air in the evening, than in the morning. I had early observed, also, bodies exposed to the sky, on a cloudy but calm night, to be sometimes 2° or 3° colder than the air, without having any appearance of dew; and when two metals possessing different relations to dew were exposed together, I have seen the one, which was the fitter to attract that fluid, colder than the other, though both were dry.

I shall conclude this part of my Essay, with relating the results of some experiments, which were made for the purpose of ascertaining the tendencies of various bodies to become cold, upon exposure to the sky at night. Unfortunately, the weather was not always favourable to my views; but what occurred appears to me, notwithstanding, worthy of being related.

In the observations hitherto given by me on the cold connected with dew, the temperature of grass has been chiefly considered, partly because my first experiments had been made upon it, and partly from a wish, which arose afterwards, to compare my own experiments with those of Mr. Six, which had been confined to that substance. I found it, however, very unfit to furnish the means of comparing the degrees of cold produced at night on the surface of the earth, at different times and places; as its state on different nights, on the same parts of the plat I commonly made use of, and in different parts of the plat on the same nights, was often very unequal, in point of height, thickness and fineness, all of which circumstances influenced the degree of cold produced by it. I observed, in consequence, a much greater uniformity in the results of experiments made with various other bodies, whose condition, when first exposed to the air, was always the same. Of these, the most productive of cold were the filamentous and downy, as wool of moderate fineness, very fine raw silk, very fine unspun cotton, fine flax, and swandown, all of which were not only more steadily cold, upon clear and calm nights, than grass, but also gave rise to a greater degree of cold, than was almost at any time observed upon it, even in its best state. Among the bodies of this class, wool produced the least cold, and I formerly mentioned that it attracted less dew, than silk, cotton, and flax. The last mentioned substances, and swandown, were found equal, or nearly so, in their tendency to become cold. Swandown, however, exhibited the greatest cold rather more frequently than any of the rest; on which account, and from its being more easily managed, as it was used while adhering to the skin of the bird, I at length scarcely ever employed any other body of the same class. On the night, during which grass was observed to be 14° colder than the air, swandown, lying upon a neighbouring piece of grass, was still one degree lower. This difference of 15°, between the temperature, at night, of a body on the surface of the earth, and that of the air, a few feet above the earth, is the greatest which I have hitherto seen.

Fresh, unbroken straw, and shreds of white paper, though not properly to be ranked among filamentous substances, were also found to be a little more productive of cold, than the wool which I used.

The next class consisted of bodies in the state of a powder, more or less fine. These were clean river sand, glass, chalk, charcoal, lampblack, and a brown calx of iron. Chalk produced the least, and the three last substances, the greatest cold. They were all, however, inferior in this respect to bodies of the first class.

Solid bodies, having a surface exposed to the sky, of at least 25 inches square, formed a third class, on which such experiments were made. The particular substances of this description, subjected to trial, were glass, brick, cork, oak-wood, and wax; all of which were, likewise, found inferior to the filamentous substances. From these last experiments it follows, that when a glass bulb of a thermometer is applied at night to a body exposed to a clear sky, the temperature exhibited by the instrument will not be accurately that of the body in question, except the disposition of the latter to become cold, in such a situation, be the same as that of glass. An example of this fact has been given in this Essay*.

My principal experiments, however, of this kind were made with snow.

On the 25th of January 1813, the ground being then covered with snow about an inch deep, I went to my usual place of experiment in the country; but, during 8 hours that I attended to my thermometers, the whole sky was constantly overcast with clouds. The atmosphere was, for the greater part of that time, very still, and a thermometer on the snow was

* Page 164.

generally about 2° lower, than another in the air. That this difference was not owing to evaporation was proved by the thermometer on the snow always rising, from a half to a whole degree, whenever the air was a little moved, and falling the same quantity, as soon as a great stillness again took place.

I had no opportunity of renewing my observations upon snow, before the beginning of the present year, 1814. The state of my health rendering it improper, that I should incur much fatigue, or be long exposed to night air, I restricted myself to the making a few experiments, in the large garden in Lincoln's-Inn Fields. I went thither, for the first time, on the evening of the 4th of January, immediately after a considerable snowfall had ceased, wishing to begin my observations, before any cold should arise on the snow's surface, from exposure to the sky. This was desirable on another account; for Mr. Kirwan, in direct opposition to indisputable facts, most clearly stated by Mr. Wilson, had said, that the great cold, observed by that gentleman on snow, was occasioned by this substance having retained the temperature of the high region, from which it had fallen*. The result of my inquiry was, that the surface of the snow, and the air 4 feet

* On Temperatures, p. 30.

above it, had precisely the same heat. The depth of the snow was 4 inches.

My next experiment took place on the evening of the 6th, the intervening day having been snowy. The sky was clear, but the air had a considerable motion. The heat of the atmosphere, at the height of 4 feet, was at 9½ h. 26°; while that of the surface of the snow, and of swandown lying upon it, was 22°. The depth of the snow was now about 5 inches.

On the 7th, a little after sunset, the heat of the air in the garden was 23°, that of the surface of snow 19°, but that of swandown lying upon the snow only 15°. There was then a gentle breeze; some parts of the sky were covered with clouds, and the lower atmosphere was a little obscure. While the exposed surface of the snow was 19°, a part of its surface, which had been covered, about 20 minutes, with a piece of pasteboard, was 22°. Grass, at the bottom of the snow, was 31°, and the earth an inch beneath the grass 32°.

After this, there was no fit time for observation until the 13th. The thermometers were exposed at 8 h. on the evening of that day, the sky being then without clouds; but the stars were not bright, and there was a perceptible motion in the air. At 8½ h. the temperature of the air was 22½°, that of the surface of the snow

13°, and that of swandown, lying on the snow, 8°. At 9 h. the air was 23½°, snow 17°, and swandown 15°. The sky being now, in great measure, covered with high thin clouds, my experiments ceased. At 10½ h. the sky was very bright, and the atmosphere very calm; but it was not then convenient to me to renew my observations. Had I repeated them at that time, I should probably have found a difference, between the temperature of the swandown and air, several degrees more considerable than the one of 14½°, which had already occurred on this evening, and consequently greater than the greatest observed by Mr. Wilson, between the temperatures of snow and of the atmosphere, which was one of 16°.

The next favourable evening was that of the 21st. Much snow having in the meanwhile fallen, its depth was now more than a foot. The thermometers were observed 5 times between 4 h. 15 m. and 4 h. 55 m. At 4 of those times, the swandown was 13°, and at one of them 13½°, colder than the air, the heat of which at the 4 first observations was 26°, and at the last 25½°. The temperature of the surface of the snow, during the whole period of observation, was 17°, and consequently 4 times it was 4°, and once 5°, less cold, than that of the swandown. The atmosphere was altogether free from clouds, and nearly quite calm, but a good deal hazy.

Before another proper evening arrived, my health became so infirm, that I was obliged to relinquish this pursuit. I conclude therefore my account of it, with two remarks. 1. If Mr. Wilson had been accustomed to examine the temperature of swandown, or any similar substance, placed upon snow, he would, probably, have observed a cold, on the surface of the earth, exceeding that of the atmosphere by 20° or more, on the night of his actually observing an excess of 16°. 2. Since upon one evening, when the atmosphere was neither very clear nor very still, a difference of $14\frac{1}{2}°$ was found by me, between the temperatures of air and of swandown, which is only $\frac{1}{2}$ a degree less than the greatest difference I have ever observed, between the same substances on the stillest and clearest nights in summer, a corroboration is hence derived of a conclusion, made by Mr. Six from his experiments, that the greatest differences at night, in point of temperature, between bodies on the surface of the earth, and the atmosphere near to it, are those which take place in very cold weather.

PART II.

OF THE THEORY OF DEW.

DEW, according to Aristotle*, is a species of rain, formed in the lower atmosphere, in consequence of its moisture being condensed by the cold of the night into minute drops. Opinions of this kind, respecting the cause of dew, are still entertained by many persons, among whom is the very ingenious Mr. Leslie of Edinburgh†. A fact, however, first taken notice of by Gersten, who published his treatise on dew in 1733, proves them to be erroneous; for he found, that bodies a little elevated in the air often become moist with dew, while similar bodies, lying on the ground, remain dry, though necessarily, from their position, as liable to be wetted, by whatever falls from the heavens, as the former.

Shortly after the appearance of Gersten's treatise, Musschenbroek made the remark, already mentioned in this Essay, that metals will be free from dew, while other bodies attract it copiously. This philosopher contented himself

* Meteor. Lib. 1. c. x. et De Mundo. c. iii.

† Relations of Heat and Moisture, p. 37, and 132.

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with publishing his discovery; but his friend Dufay concluded from it, that dew is an electric phenomenon, since it leaves untouched the bodies, which conduct electricity, while it appears upon those, which cannot transmit that influence. If dew, however, were to form on the latter only, its quantity would never be sufficiently great, to admit its being distinctly seen; for the non-conductors, as soon as they became in the least moist, would be changed into conductors. Charcoal, too, it is now known, though the best solid conductor of electricity after the metals, attracts dew very powerfully; and, in the last place, contrary to the assertion of Dufay, dew frequently forms upon metals themselves.

Other authors have ascribed the production of dew to electricity, for reasons different from that of Dufay. But there are several considerations, which seem to me to prove, that no such opinion can be just. 1. When dew is produced in a clear atmosphere, the portion of air, by which it is deposited, must necessarily be unable, at that moment, to retain, in a state of pellucid vapour, all the moisture, which it had immediately before held in that form. But I know of no experiment, which shows, that air, by becoming positively electrical, which is said to be its condition on the evenings, during which dew is most abundant, is rendered less able, than it had previously been, to contain watery vapour in a state of transparency. 2. Bodies in similar circumstances, as far as electricity is concerned, acquire very different quantities of dew. Wool placed on the raised board, for example, attracted very much more dew, than wool attached to the lower side of the same board, and even considerably more than the same substance freely suspended in the air, and entirely exposed to the sky. 3. Dew forms in different parts of the night, in quantities no way proportioned to the degrees of electricity found in the atmosphere at the same times. Thus, it is commonly more copious in the morning than in the evening, notwithstanding that the air is observed to be, in the latter season, more highly electrical than in the former. 4. I have several nights held a glass bottle, upon which dew was forming, close to the top of a Bennett's electrometer, which had been previously kept in a dry place; but I never saw the slips of gold leaf to move in consequence. It is very probable, however, that more refined experiments will show, that electrical appearances attend the production of dew. These, perhaps, accompany every change in the chemical form of bodies. But the facts, which have been stated, seem sufficient to establish, that any such appearances, which may be

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hereafter remarked, during the formation of dew, must be considered as effects, and not as the cause, of the conversion of the watery vapour of a clear atmosphere into a fluid.

A remaining argument applies equally to all the theories, which have hitherto been made public on the cause of dew. This is, that none of them include the important fact, that its production is attended with cold; since no explanation of a natural appearance can be well founded, which has been built without the knowledge of one of its principal circumstances. It may seem strange to many, that neither Mr. Wilson, nor Mr. Six, applied this fact to the improvement of the theory of dew. But according to their view of the subject, no such use could have been made of it by them, as they held the formation of that fluid to be the cause of the cold observed with it. I had many years, as was formerly mentioned, held the same opinion; but I began to see reason, not long after my regular course of experiments commenced, to doubt its truth, as I found that bodies would sometimes become colder than the air, without being dewed; and that, when dew was formed, if different times were compared, its quantity, and the degree of cold which appeared with it, were very far from being always in the same proportion to each other. The frequent recurrence of such observations at length converted the doubt of the justness of my ancient opinion, into a conviction of its error, and at the same time occasioned me to conclude, that dew is the production of a preceding cold in the substances, upon which it appears. Wishing, however, to obtain proofs, more striking in degree, of the validity of these inferences, than such as had been afforded to me by casual observation, while attending to other parts of my subject, I instituted the experiments which will be next related.

I had frequently remarked, early in the evening, a considerable degree of cold on substances exposed in calm weather to a clear sky, and I had also sometimes seen, early in the evening, the raised board altogether dry, while the grass was much moistened. I therefore determined to make the experiments in view on the raised board, and to commence them as soon as the sun should cease to shine upon it. The first day I went to the country for this purpose, the 19th of August 1813, almost every circumstance was favourable to its completion. There had been no rain for three weeks; the wind was northerly; and the barometer was rising; all which indicated, that the atmosphere contained little moisture. The air too was extremely still. The only appearance in the least unfavourable was, that the sky was not entirely free from clouds; but these were few, of small extent, thin, and high.

At 6 h. 25 m. immediately after the sun had ceased to shine upon the spot, where my experiments were to be carried on, though the time of its setting was still 47 minutes distant, I placed upon the raised board 10 grains of wool, and a small bag, made of the skin of a swan's breast with the down adhering, and stuffed with wool, the whole weighing nearly 5 drachms. On each of these substances the naked bulb of a small and delicate thermometer was laid. A similar thermometer, with its bulb also naked, was suspended in the air, over the grassplat, at the same height with the board. Two thermometers were placed in other situations, as will be seen in the annexed Table. After an exposure of 20 minutes, the wool was 7° colder than the air, but the swandown bag only 6°, no doubt in consequence of its comparatively great quantity of matter. Neither, however, had gained the least weight, according to the scales employed by me, which were sensibly moved by the 16th of a grain. These observations were repeated several times during the following hour, as will be seen by the Table, at none of which, except the last, was either the wool or swandown found in the least heavier, than when first placed on the board. At this last observation, the wool, though $9\frac{1}{2}°$ colder than the air, was still without any increase in weight; but the swandown, which was 1° colder than the wool, had gained $\frac{1}{2}$ a grain. My experiments now properly ceased; but having suffered the thermometers, which had been placed on the wool and swandown, and in the air, to remain in those situations, I examined them again at 8 h. 45 m., that is, 2 h. 20 m. after they had been first exposed. The wool, which was still $9\frac{1}{2}°$ colder than the air, had gained somewhat less than $\frac{1}{2}$ a grain; and the swandown, which was now $11\frac{1}{2}°$ colder than the air, had gained 2 grains, including the $\frac{1}{2}$ grain already mentioned. When these last observations were made, the sky was entirely cloudless, and the atmosphere very calm.

TABULAR VIEW OF OBSERVATIONS
on the Evening of August 19, 1813.

	6h. 45m.	7h.	7h. 20m.	7h. 40m.	8h. 45m.
Heat of air 4 feet above the grass	$60\frac{1}{2}°$	$60\frac{1}{2}°$	59°	58°	54°
——— wool on the raised board	$53\frac{1}{2}$	$54\frac{1}{2}$	$51\frac{1}{2}$	$48\frac{1}{2}$	$44\frac{1}{2}$
——— swandown on the same	$54\frac{1}{2}$	53	51	$47\frac{1}{2}$	$42\frac{1}{2}$
——— surface of the raised board	58	57	$55\frac{1}{2}$		
——— grassplat*	53	51	$49\frac{1}{2}$	49	42

* In these experiments, contrary to what usually happens, the grass was almost constantly colder than the filamentous substances, although they were placed upon the raised board.

Similar experiments made at the same place, on the evenings of the 25th of August and 17th of September, in the same year, had results, which were also similar but less in degree; the greatest difference between the temperature of wool or swandown, while they were without any increase of weight, and the temperature of the air, having been, on the first of those evenings, only 4°, and on the second only 5°. The reasons were, in great measure, if not wholly, that a considerable part of the sky was covered with clouds, and that the air was commonly in that state of motion, which is denominated a gentle breeze.

On the evening of my first experiments, I had omitted to measure the heat of the raised board, before the thermometers were placed upon it. This was attended to on the two latter evenings, on the first of which its upper surface was found, at the commencement of the experiments, 4° warmer than the air; on the second, both it and the air were of the same temperature. Again; on the first of the latter evenings, 10 grains of wool, to which 3 grains of water had been added, having been laid on the raised board, near the thermometers; at the end of 45 minutes the parcel was found to have lost $2\frac{1}{2}$ grains of moisture by evaporation, during the time, that dry wool had become several degrees colder than the air.

A fourth experiment of this kind was made by me on the 7th of January, 1814, in the garden of Lincoln's-Inn-Fields, by placing 10 grains of wool on a sheet of pasteboard, which lay upon the snow. At the end of 35 minutes the wool was 5° colder than the air, without possessing any additional weight.

Having thus shown the justness of my former conclusion, that the cold, observed with dew, is the previous occurrence, and, consequently, that the formation of this fluid has precisely the same immediate cause, as the presence of moisture upon the outside of a glass or metallic vessel, when a liquid considerably colder than the air has been poured into it shortly before; I shall next apply this fact to the explanation of several atmospherical appearances.

I. The variety in the quantities of dew, which were found by me upon bodies of the same kind, exposed to the air during the same time of the night, but in different situations, is now seen to have been occasioned by the diversity of temperature, which existed among them.

II. Agreeably to the opinion of Mr. Wilson and Mr. Six, the cold connected with dew ought always to be proportional to the quantity of that fluid; but this is contradicted by experience. On the other hand, if it be granted, that dew is water precipitated from the atmosphere, by the cold of the body on which it appears, the same degree of cold, in the precipitating body, may be attended with much, with little, or with no dew, according to the existing state of the air in regard to moisture; all of which circumstances are found actually to take place.

III. The formation of dew, indeed, not only does not produce cold, but like every other precipitation of water from the atmosphere, produces heat. I infer this, partly because very little dew appeared upon the two nights of the greatest cold I have ever observed on the surface of the earth, relatively to the temperature of the air, both of them having occurred after a long tract of dry weather; and partly from the most dewy night, which I have ever seen, having been attended, during the greater part of it, with no considerable degree of cold. On this night, the difference between the temperatures of grass and of air was at first $7\frac{1}{2}°$, the dew being then not very abundant. But, after the dew had become very abundant, the difference of those temperatures never exceeded 4°, and was frequently only 3°.

With the view of obtaining, though indirectly, some knowledge of the quantity of cold, which had been prevented, by the formation of dew, from appearing on the surface of the earth, in the night just spoken of, I made the following experiment. To 10 grains of wool having the same form and extension, as the parcels employed for the collection of that fluid, were added 21 grains of water, this being the quantity of moisture, which had been attracted by 10 grains of wool, lying on the grassplat, in the space of 8 hours on that night. The wet wool having been then placed in a china saucer, laid on a feather-bed in a room, the door and windows of which were shut, its heat during the following 8 hours was, at frequent examinations, uniformly found to be about 4° less, than that of a dry china saucer on the same bed; the temperature of the air in the room not having altered more than ½ a degree, in the course of the experiment. At the end of the 8 hours, the wool still retained 2½ grains of moisture. If this quantity had also evaporated, the cold uniformly produced during the 8 hours would, in all probability, have been about 4½°. From this experiment, therefore, I think it may be inferred, that the mean quantity of cold, which was prevented, by the formation of dew, from appearing on the ground, during the night which has been mentioned, was also about 4½°. But, as the production of dew, during some parts of the night, was at a greater rate, than that of 21 grains for 8 hours, 1 or 2 degrees may be added for those times, which will raise the effect of the dew in diminishing the appearance of cold during them to about 6°, on the supposition, which cannot be far from the truth, that dew had been attracted as copiously by the grass, as by wool which lay upon it.

The less difference commonly observed between the temperatures of grass and of air, in the morning, than what occurs in the evening, is likewise to be, in part, attributed to a greater quantity of dew appearing in the former, than in the latter season.

A more remarkable fact, deriving an explanation from the same source, is the greater difference which takes place in very cold weather, if it be calm and clear, between the temperatures of the air and of bodies on the earth, at night, than in equally clear and calm weather in summer; since, in very cold weather, any diminution of the temperature of a portion of air, in contact with a cold body, will be attended, in consequence of the well known relations of the atmosphere to moisture, with a much less formation of water, than an equal diminution would be in summer, supposing the air, before it touches the cold body, to be at both times equally near to its point of repletion with moisture.

IV. In very calm nights, a portion of air, which comes in contact with cold grass, will not, when the surface is level, immediately quit it, more especially, as this air has become specifically heavier than the higher, from a diminution of its heat, but will proceed horizontally, and be applied successively to different parts of the same surface. The air, therefore, which makes this progress, must at length have no moisture to be precipitated, unless the cold of the grass which it touches should increase. Hence in great measure is to be explained, why on such nights, as have been just mentioned, more dew was acquired by substances placed on the raised board, than by others of the same kind on the grass, though it began to form much sooner in the latter than in the former situation, those on the raised board having received air, which had previously deposited less of its moisture.

A reason is now also afforded, why a slight agitation of the atmosphere, when very pregnant with moisture, should increase the quantity of dew; since fresh parcels of air will hence be more frequently brought into contact with the cold surface of the earth, than if the atmosphere were entirely calm.

V. Dew, in agreement with the immediate cause which has been assigned by me for its production, can never be formed, in temperate climates, upon the naked parts of a living and healthy human body, during the night; since their heat is never less in this season, in such climates, than that of the atmosphere. I have, in fact, never perceived dew on any naked part of my own body at night, though my attention was much occupied, for three years, with every thing relative to this fluid, and though I had been, during that period, much exposed to the night air. On the other hand, in very hot countries, the uncovered parts of a human body may sometimes, from being considerably colder than the air, condense the watery vapour of the atmosphere, and hence be covered with a real dew, even in the day-time.

VI. Hygrometers formed of animal or vegetable substances, when exposed to a clear sky at night, will become colder than the atmosphere; and hence, by attracting dew, or, according to an observation of Saussure*, by merely cooling the air contiguous to them, mark a degree of moisture, beyond what the atmosphere actually contains. This serves to explain an observation made by Mr. De Luc†, that in serene and calm weather, the humidity of the air, as determined by an hygrometer, increases about,

* Hygronometrie, p. 25.

† Introduction à la Physique Terrestre, II. 491.

and after sunset, with a greater rapidity, than can be attributed to a diminution of the general heat of the atmosphere.

These examples are sufficient to show the value of the fact, that bodies become colder than the neighbouring air, before they are dewed, in explaining many atmospherical appearances. To this point, the investigation of the cause of dew might have been carried at any time, since the invention of thermometers; but its complete theory could not possibly, in my opinion, have been attained, before the discoveries on heat were made, which are contained in the works of Mr. Leslie and Count Rumford.

The experience of most persons, respecting the communication of heat among bodies in the open air, is confined to what happens during the day; at which time, those that are situated near to one another are always found to possess the same temperature, unless some very evident reason for the contrary should exist. To many, therefore, it may appear incredible, that a perfectly dry body, placed in contact, on all sides, with other bodies of the same temperature with itself, shall afterwards, without undergoing any chemical change, become much colder than they are, and shall remain so for many hours; yet these circumstances are found to occur in substances attractive of dew, when laid on the surface of the earth, in a still and serene night, and are in perfect agreement with the doctrine of heat, now universally admitted to be just.

To render this more easy of apprehension, let a small body which radiates heat freely, and possesses a temperature, in common with the atmosphere, higher than 32°, be placed, while the air is clear and still, on a slow conductor of heat lying on the surface of a large open plain, and let a firmament of ice be supposed to exist at any height in the atmosphere; the consequence must be, that the small body will, from its situation, quickly become colder than the neighbouring air. For, while it radiates its own heat upwards, it cannot receive a sufficient quantity from the ice to compensate this loss; little also can be conveyed to it from the earth, as a bad conductor is interposed between them; and there is no solid, or fluid except the air, to communicate it laterally either by radiation or conduction. This small body, therefore, unless it shall receive from the air, nearly as much heat as it has emitted, which, considering the little that can be communicated from one part of the atmosphere to another, in its present calm state, must be regarded as impossible, will become colder than the air, and condense the watery vapour of the contiguous parts of it, if they should contain a sufficient quantity to admit of this effect. But events similar to these occur, when dew appears in an open and level grass field, during a still and serene night. The upper parts of the grass radiate their heat into regions of empty space, which consequently send back no heat in return; its lower parts, from the smallness of their conducting power, transmit little of the earth's heat to the upper parts, which at the same time receiving only a small quantity from the atmosphere, and none from any other lateral body, must remain colder than the air, and condense into dew its watery vapour, if this be sufficiently abundant, in respect to the decreased temperature of the grass*.

This subject may be further illustrated by a reference to what happens in the experiment, which has been used to prove the reflection of cold.

In the simplest form of this experiment, a

* I have adopted in this explanation the hypothesis of Mr. Prevost of Geneva, on the constant radiation of heat by bodies in contact with the atmosphere, even at the time that they are exposed to the influence of bodies warmer than themselves; as it appears to agree perfectly with all the phenomena of the communication of heat, which do not depend upon conduction. I shall hereafter make frequent use of this hypothesis.

small body, the bulb of a thermometer, possessing the temperature of the atmosphere, is placed before a larger cold body, rendered equal in effect to one still larger, by means of a concave metallic mirror. In this situation, the small body radiates heat to the larger, without receiving an equivalent from it, and, in consequence, becomes colder than the air through which its heat is sent, notwithstanding that it is continually gaining some heat, both from the air which surrounds it, and from the walls and contents of the apartment, in which the experiment is made. Dew, therefore, would as readily form upon the thermometer in this experiment, as it would upon one suspended in the open air at night, under a clear sky, provided that the two instruments were equally colder than the atmosphere, and that this was in both cases equally near to being replete with moisture*.

* The invention of this experiment having been ascribed a few years ago to Mr. Pictet of Geneva, various English writers have shown, that it occurs in several much older foreign authors. But I have not seen any mention made of its having been also long since known in this country. That it was so appears from the following extract of a letter, written by Mr. Oldenburgh to Mr. Boyle in 1665. "I met the other day in the Astrological Discourse of Sir Christopher Heydon, with an experiment, which he affirms to have tried himself, importing, that cold accompanies reflected light, by employing burning spherical concaves, or parabolical sections,

Regarding now as established, that bodies situated on or near to the surface of the earth become, under certain circumstances, colder than the neighbouring air, by radiating more heat to the heavens, than they receive in every way*, I shall in the first place offer a few remarks on the extent and use of this occurrence, and shall afterwards apply the knowledge of it to the explanation of several more of the appearances described in the former part of this Essay, and of some others, which have not hitherto been mentioned by me.

which, he saith, will as sensibly reflect the actual cold of snow or ice, as they will the heat of the sun." Boyle's Works, folio, vol. V. p. 345.

* Count Rumford offered the following conjecture, in a paper printed in the Philosophical Transactions for 1804. "The excessive cold which is known to reign, in all seasons, on the tops of very high mountains, and in the higher regions of the atmosphere, and the frosts at night, which so frequently take place on the surface of the plains below, in very clear and still weather, in spring and autumn, seem to indicate, that frigorific rays arrive continually at the surface of the earth, from every part of the heavens." But he gave no experiments to prove, that such a communication actually exists between the heavens and the earth at night. Neither does it appear from any of his writings which I have seen, that he ever supposed, that the surface of the earth is more cooled by these frigorific rays, than the air through which they pass, or that some solid bodies are more cooled by them than others.

Radiation of heat by the earth to the heavens must exist at all times; but, if the sun be at some height above the horizon, the degree of which is hitherto undetermined, and probably varies according to season, and several other circumstances, the heat emitted by it to the earth will overbalance, even in places shaded from its direct beams, that which the earth radiates upwards. I suspended at midday, on the 24th of July, 1813, in the open air over a grassplat, while the sky was wholly covered with very dense clouds, and the weather calm, two delicate thermometers, one of which was naked, but the other cased in gold paper. At two observations, having an interval of 10 minutes between them, the thermometer in the gilt case was 2° lower than that which was naked. A white paper case was then drawn over the gilt one, upon which, after 5 minutes, the covered instrument was observed to be at the same height with the naked. The outer white case having, in the next place, been taken from the covered thermometer, but that which was gilt suffered to remain, the two instruments were in a few minutes found again to differ 2°. A thermometer on the grassplat was, during these experiments, higher than the naked instrument in the air by 2°, and than that in the gilt case by 4°. It is evident, therefore, that heat radiated by the sun must, on this day, have been transmitted in considerable quantity through the thickest clouds; since not only was the earth's surface warmer than the air, but a small body, covered with a substance not readily admitting the entrance of radiant heat, was colder than a similar body which was uncovered. In like manner, I observed at noon, on the 2nd of January, 1814, during the prevalence of a dense fog, a thermometer placed upon swandown, which was lying upon grass thickly incrusted with hoarfrost, to be 2° warmer than the air, and 1° warmer than the grass*.

In a calm and serene night, however, when consequently little impediment exists to the escape, by radiation, of the earth's heat to the heavens, and when no heat can be radiated by the sun to the place of observation, an immense degree of cold would occur on the ground, if the following circumstances did not combine to lessen it. 1. The incapacity of all bodies to prevent, entirely, the passing of heat, by conduction, from the earth to substances placed upon them. 2. The heat radiated to these

* Another fact of the same kind, which occurred at the same time, is that, although the temperature of the air was 30°, the hoarfrost on trees rapidly decreased, the solid matter of the trees intercepting radiant heat, which had penetrated through the fog from the sun, and converting it into heat of temperature.

substances by lateral objects. 3. The heat communicated to the same substances by the air. 4. The heat which is evolved, during the condensation of the watery vapour of the atmosphere into dew.

The extent of the effect of all these checks upon the production of cold, by the nightly radiation of heat from bodies on the surface of the earth, cannot, in the present state of our knowledge, be properly estimated; but facts show that, notwithstanding their operation, the cold originating in this source must be often very considerable.

1. Mr. Wilson once observed a difference of 16°, from this cause, between the temperatures of snow and of air. In taking the latter temperature, however, he employed a naked thermometer, on which account, in consequence of what has already been mentioned by me, about 2° are to be added to the 16° noted by him, in order to obtain the real difference between the heat of the snow and the air at that time*.

2. If Mr. Wilson, as was formerly said, had

* As bright metals, when suspended in the air, and exposed to a clear sky on a calm night, become colder than the surrounding atmosphere, a thermometer covered with metalled paper, and placed in the circumstances which have been just mentioned, will mark a temperature less than that of the air near to it. But, as the difference must be small, and as I know of no way to estimate it accurately, I have hitherto always neglected to consider it.

laid a thermometer on any downy substance in contact with the snow, he would, in all probability, have found a cold indicated by it at least 20° greater than that of the air, as marked by a naked instrument, and consequently at least 22° greater than the real cold of the surrounding atmosphere.

3. Mr. Wilson's place of observation was not very favourable to the occurrence of a great cold, from radiation of heat at night, it being near to a large smoky city, in the immediate vicinity also, as appears to me from what he says of it, of one or more considerable buildings, and in a climate abounding in moisture.

4. None of Mr. Wilson's experiments, in which a very great degree of cold occurred, were made within an hour or two after sunset, during which time, according to my observation, the most considerable differences between the temperatures of the air, and of bodies on the surface of the earth, commonly happen.

If, then, such experiments should be made in an atmosphere still colder than that, in which Mr. Wilson made his, on a large plain remote from any city, and free from objects of every kind, that are elevated above the ground, and in a country remarkable for the dryness of its air, all which circumstances may be found in Russia during the winter; a difference of at least 30° would probably appear, on some still and serene night, between a small thermometer placed with its bulb naked*, on the middle, or leeward side of a stratum of a downy substance, occupying a space upon a grass field, or bed of snow, one or two square yards in extent, and a similar thermometer inclosed in a case of gilt paper, and suspended in the air a few feet above the other. Two thermometers, thus placed, would, I think, be sometimes found even in this country to differ not much less than 30°. I have myself never made any such experiments with a downy substance, which had a surface of more than a few square inches, or in a very cold night, when the atmosphere was clear and calm, and the scene of observation remote from large masses of building.

But even a cold of 30° appears not to be the greatest, that can be thought to occur, from the radiation of heat to the heavens, at night, by substances on the surface of the earth. For experiments by Mr. Pictet†, Mr. Six‡, and I may add by myself, establish that, in exception to the common rule, the heat of the atmosphere in clear and calm nights *increases* with the distance

* The effect would, perhaps, be a little increased, by covering the bulb with a very thin layer of lamp-black.

† Essai sur le Feu, c. x.

‡ Phil. Trans. 1784, and 1788.

from the earth. Agreeably to Mr. Six's experiments, the atmosphere at the height of 220 feet is often, upon such nights, 10° warmer than what it is 7 feet above the ground. If, therefore, I am able to show, as I expect I shall be in the course of a few pages, that the air at the smaller height becomes colder than that of the greater, from its vicinity to the surface of the earth, previously rendered cold by radiating its heat to the heavens, it will follow, that these 10° must be added to the quantity of cold already mentioned; and, consequently, that a body on the ground may become, at night, at least 40° colder than the air two or three hundred feet above it, by the radiation of its heat to a clear sky.

I shall add, with the greatest diffidence, a few words upon a final cause of the radiation of heat from the earth at night, and upon some of the circumstances which modify its action, though fully conscious of the danger of error, which is always incurred in the attempt to appreciate the works of our Creator.

The heat which is radiated by the sun to the earth, if suffered to accumulate, would quickly destroy the present constitution of our globe*. This evil is prevented by the radiation of heat

* Count Rumford says; "May it not be by the action of these [frigorific] rays, that our planet is cooled continually, and enabled to preserve the same mean temperature for ages,

by the earth to the heavens, during the night, when it receives from them little or no heat in return. But, through the wise economy of means, which is witnessed in all the operations of Nature, the prevention of this evil is made the source of great positive good. For the surface of the earth, having thus become colder than the neighbouring air, condenses a part of the watery vapour of the atmosphere into dew, the utility of which is too manifest to require my speaking of it. I may remark, however, that this fluid appears chiefly where it is most wanted, on herbage, and low plants, avoiding, in great measure, rocks, bare earth and considerable masses of water*. Its production too,

notwithstanding the immense quantities of heat that are generated at its surface, by the continual action of the solar rays?" Phil. Trans. 1804, p. 181.

* I have no direct observations for the foundation of this assertion concerning considerable masses of water. But, I hold it, notwithstanding, to be just; because, as soon as the surface of the water is in the least cooled by radiation, the particles composing it must fall downwards, from their increased gravity, and be replaced by others that are warmer. The whole mass, therefore, can never, in the course of a single night, be sufficiently cooled to condense into dew any great quantity of the watery vapour of the atmosphere. Besides; I have found, that even a small mass of water, as will be more particularly mentioned in the last part of this Essay, sometimes acquires no weight from the reception of dew, in the space of a whole night favourable to the formation of that fluid.

by another wise arrangement, tends to prevent the injury, that might arise from its own cause; since the precipitation of water, upon the tender parts of plants, must lessen the cold in them, which occasions it. I shall observe in the last place, that the appearance of dew is not confined to any one part of the night, but occurs during its whole course, from means the most simple and efficacious. For after one part of the air has deposited its moisture, on the colder surface of the earth, it is removed, in consequence of that agitation in the atmosphere which exists during its stillest states, and gives place to another having its quantity of water undiminished; and, again, as the night proceeds, a portion of air, which had before deposited all the moisture, which circumstances at that time permitted, is rendered fit, by the general increase of the cold of the atmosphere, to give out a fresh parcel, when it comes anew into contact with the ground.

I. The first fact, which I shall here attempt to explain, is the prevention, either wholly or in part, of cold, from radiation, in substances on the ground, by the interposition of any solid body between them and the sky. This evidently appears to arise in the following manner. The lower body radiates its heat upwards, as if no other intervened between it and the sky;

but the loss, which it hence suffers, is more or less compensated by what is radiated to it, from the body above, the under-surface of which possesses always the same, or very nearly the same temperature as the air. In this way therefore, is to be accounted for the warmth of the substances, which were sheltered from the sky by the raised board, the pasteboard roof, and the hollow cylinders of earth and pasteboard. In these examples, the interposed substances cannot be supposed to have remitted more heat than they received. But in situations where large masses of bare solid matter exist, which are warmer than the atmosphere, from the heat of the preceding day or other causes, a greater heat will be received by the exposed body, than what is radiated by itself. For example, it seems certain to me, that the houses, surrounding Lincoln's-Inn Fields, had an influence upon my thermometers, during my experiments there at night, beyond what arose from their merely returning a quantity of heat, equivalent to that, which they received from the surface of the garden. It is not, however, absolutely requisite, that a body should be itself exposed to the sky on a clear and calm night, in order to become colder than the atmosphere; exposure to the influence of another body, so situated, is sufficient for the production of a slight degree of this effect. Thus, I have always found wool attached to the underside of my raised board, on such a night, to be a little colder than the air; and it has appeared to me a sufficient reason for the fact, that the wool in this situation was, in some degree, exposed to the influence of grass, which had become considerably colder than the atmosphere, by radiating its heat to the sky.

II. No direct experiments can be made to ascertain the manner, in which clouds prevent, or occasion to be small, the appearance of a cold at night, upon the surface of the earth, greater than that of the atmosphere; but it may, I think, be firmly concluded, from what has been said in the preceding article, that they produce this effect, almost entirely, by radiating heat to the earth, in return for that which they intercept in its progress from the earth towards the heavens. For although, upon the sky becoming suddenly cloudy during a calm night, a naked thermometer, suspended in the air, commonly rises 2 or 3 degrees, little of this rise is to be attributed to the heat evolved by the condensation of watery vapour in the atmosphere, as was supposed by Mr. Wilson *; since, in consequence of the ceasing of that part of the cold indicated by the thermometer, which

* Edin. Phil. Trans. I. 157.

was owing to its own radiation to a clear sky, the temperature of the atmosphere may seem to increase 2°, or more, notwithstanding that it has received no real addition. Besides; the heat which is extricated by the condensation of vapour, during the formation of a cloud, must soon be dissipated; whereas the effect of greatly lessening, or preventing altogether, the appearance of a superior cold on the earth to that of the air, will be produced by a cloudy sky, during the whole of a long night.

Dense clouds, near the earth, must possess the same heat as the lower atmosphere, and will therefore send to the earth, as much, or nearly as much heat as they receive from it by radiation. But similarly dense clouds, if very high, though they equally intercept the communication of the earth with the sky, yet being, from their elevated situation, colder than the earth, will radiate to it less heat than they receive from it, and may, consequently, admit of bodies on its surface becoming several degrees colder than the air. In the first part of this Essay, an example was given of a body on the ground becoming at night 5° colder than the air, though the whole sky was thickly covered with high clouds*.

* Mr. Prevost of Geneva, in his work on Radiant Heat, p. 382, has already in this way, conjecturally, accounted for the effect of clouds, in diminishing, at night, the cold of the

Islands, and parts of continents close to the sea, being, by their situation, subject to a cloudy sky, will, from the smaller quantity of heat lost by them through radiation to the heavens at night, in addition to the reasons commonly assigned, be less cold in winter, than countries considerably distant from any ocean.

III. Fogs, like clouds, will arrest heat, which is radiated upwards by the earth, and, if they be very dense, and of considerable perpendicular extent, may remit to it as much as they receive. Accordingly, Mr. Wilson found no

atmosphere, and of the surface of the earth; but he seems not to have known, that their effect on the temperature of the latter is much greater than that which they produce upon the air. My explanation of this influence of clouds, on the temperature of the surface of the earth, during the night, is a direct consequence from the facts, which I had observed respecting the prevention of cold on the ground from radiation, by the interposition of solid bodies between it and the heavens, and occurred to me in 1812. Mr. Prevost's work, indeed, was published in 18 9; but I did not see it before the summer of 1813; when it was lent to me by his relation Dr. Marcet of London, who at the same time said, that he believed there was no other copy of it in Great Britain, except one, which had been sent by himself to Edinburgh.

Note to second edition.] I did not know, until after the first edition of this Essay was printed, that Mr. Prevost had published his opinion on the effect of clouds in preventing the occurrence of cold at night in the atmosphere, and upon the surface of the earth, as early as 1792, in a work entitled 'Recherches sur la Chaleur.'

difference at night, in very foggy weather, between the temperature of the surface of snow, and that of the air. Several observations by myself tend to confirm that of Mr. Wilson. An instance, however, as was formerly said, occurred to me of a difference at night of 9° between the temperatures of grass crusted over with hoarfrost, and of air, during a very dense fog. A fact, remarked by Mr. Leslie, respecting fogs, serves to explain this apparent anomaly. For it was found by that philosopher*, from experiments made with his photometer, that in mists and low fogs the diminution of the sun's heat is small, when compared with what occurs, when the sky is obscured by a dense body of clouds; and it will, I presume, be readily granted, that the same state of the atmosphere, which allows the heat of the sun to pass copiously, will also give a ready transit to heat radiated by the earth. Now there are several reasons for believing, that the fog, during which grass was 9° colder than the air, did not ascend far above the ground. 1. The barometer had been falling for some days before, and it is a matter of common observation, that great fogs seldom occur, except it be high. 2. On the day preceding the observation, the

* On Heat and Moisture, p. 57.

air, after having been extremely foggy for nearly a week, had become clear enough to allow the sun's being distinctly seen during the whole of the afternoon, though there was still a sufficient obscurity in the lowermost parts of the atmosphere, to obstruct considerably the view of objects on the ground and very near to it. 3. On the day following the observation, the fog was again much less; on the next it disappeared, and was succeeded by snow. It is to be mentioned likewise, that on the evening in question the state of the grass, which was the subject of experiment, was unusually favourable to the production of cold; since, contrary to general experience, it was as cold as swandown. If, then, the latter substance, from the much greater regularity of the appearances exhibited by it, be taken as the standard, by which the occurrences of different nights are to be compared together, it will follow, that the fog of which I am speaking, though it did not prevent, must have lessened, the production of cold from radiation. For, on the preceding evening, when there was little fog, the atmosphere being equally still on both, the difference between swandown and the air was 12°; and on another, a fortnight after, the difference at the same place of observation, between thermometers in the same situations, was

$14\frac{1}{2}°$, the air being now free from fog. If the atmosphere had been as still on this, as on the former evenings, a greater difference would doubtless have been seen. I conclude, therefore, that fogs do not in any instance furnish a real exception to the general rule, that whatever exists in the atmosphere, capable of stopping or impeding the passage of radiant heat, will prevent or lessen the appearance at night of a cold on the surface of the earth, greater than that of the neighbouring air.

It follows also, from what has been said in this article, that the water deposited upon the earth, during a fog at night, may sometimes be derived from two different sources, one of which is a precipitation of moisture from a considerable part of the atmosphere, in consequence of its general cold; the other, a real formation of dew, from the condensation, by means of the superficial cold of the ground, of the moisture of that portion of the air, which comes in contact with it. In such a state of things, all bodies will become moist, but those especially, which most readily attract dew in clear weather*. I have had no opportunity, however, of trying

* The moisture observed at night by Musschenbroek in Holland, and called by him dew, appears to me to have been of this kind. See this Essay, p. 127.

this conclusion by the test of observation, since it occurred to me.

IV. When bodies become cold from radiation, the degree of effect observed must depend, not only on their radiating power, but in part also on the greater or less ease, with which they can derive heat, by conduction, from warmer substances in contact with them. Thus grass, on a clear and still night, was constantly colder, sometimes very much colder, than the gravel walk, though a small quantity of sand, placed upon grass, was always nearly as cold as this substance. In this case, the difference in temperature, between the gravel walk and sand, evidently depended on the different quantities of heat, which they received from the parts beneath. A like reason is to be given for dew appearing in greater quantity on shavings of wood, than on the same substance in a more dense and compact form; and for filamentous and downy substances becoming colder than all others, even than lampblack, which is placed by Mr. Leslie, at the head of the best solid radiators of heat. For the lampblack exposed by me, being about 2 lines in depth, possessed, in consequence, a fund of internal heat, which would more readily pass to its cold surface, than the heat of the lower parts of the downy substances would to their upper surface.

This subject is illustrated by the following experiment. On a dewy evening, I depressed into soft garden mould a drinking glass, having a thick flat bottom, until its brim was upon a level with the surrounding earth, and at the same time placed a similar vessel, with its cavity also towards the sky, on the surface of the mould. In the morning, the inside of the depressed glass was entirely dry, while that of the other was dewed. I then applied the bulb of a small thermometer to the inside of the bottom of each vessel, on which I found the heat of that part of the depressed one to be 56°, but of the same part of that which stood on the mould only 49½°. At this time the temperature of the air was 53°. The cause, therefore, was evident, both of the wetness of the first vessel, and of the dryness of the second.

From this source also is to be derived the reason, why the prominent parts of various bodies were observed by Mr. Wilson to be crusted with hoarfrost, while their more retired and massy parts were free from it *.

V. Bodies, exposed in a clear night to the sky, must radiate as much heat to it during the prevalence of wind, as they would do if the air were altogether still. But in the former case,

* Paper in Phil. Trans. 1780.

little or no cold will be observed upon them above that of the atmosphere, as the frequent application of warm air must quickly return a heat equal, or nearly so, to that which they had lost by radiation. A slight agitation of the air is sufficient to produce some effect of this kind; though, as has already been said, such an agitation, when the air is very pregnant with moisture, will render greater the quantity of dew, one requisite for a considerable production of this fluid being more increased by it, than another is diminished.

VI. A small body, as a thermometer, suspended in the air, will even in the calmest night exhibit but little cold from radiation, since it is continually exposed to the application of fresh parcels of warmer air, both from the progressive motion of this fluid, and from the downward motion produced in it by the superior gravity of such portions, as have been cooled by contact with the suspended body. On the other hand, a thermometer upon a board, raised above the earth and possessing a surface of several square yards, will have its cold from radiation much less diminished than the former, as it is exposed to no loss from a downward motion of the air, and as the air, which approaches it horizontally, must, almost always, have had its temperature previously lowered, by passing over another part of the board. The reason then of the lee side of the raised board being often colder than the windward is obvious.

VII. There is a remark by Theophrastus *, which has been confirmed by other writers, that the hurtful effects of cold occur chiefly in hollow places. If this be restricted to what happens on serene and calm nights, and it does not, I believe, hold true in any other circumstances, two reasons from different sources are to be assigned for it. The first is, that the air being stiller in such a situation, than in any other, the cold, from radiation, in the bodies which it contains, will be less diminished by renewed applications of warmer air; the second, that from the longer continuance of the same air in contact with the ground, in depressed places than in others, less dew will be deposited, and therefore less heat extricated during its formation. It will be seen in the last part of this Essay, that, in the East Indies, depressions in the earth are artificially made, for the purpose of increasing the cold, which appears in serene nights. On this subject, however, it is to be observed, that if the depressed or hollow places be deep, in proportion to their horizontal extent, a contrary effect must follow; as a case

* Lib. v. c. xvi.

will occur more or less similar to that which existed in some experiments formerly related by me, in which a small portion of grass was surrounded by a hollow cylinder.

VIII. An observation closely connected with the preceding, namely that, in clear and still nights, frosts are less severe upon hills, than in neighbouring plains*, has excited more attention, chiefly from its contradicting what is commonly regarded an established fact, that the cold of the atmosphere always increases with the distance from the earth. This inferior cold of hills is evidently a circumstance of the same kind, with that ascertained by Mr. Pictet and Mr. Six, respecting the increasing warmth, in clear and calm nights at all seasons of the year, of the different strata of the atmosphere, in proportion as these are more elevated above the earth. As the greater cold of the lower air is the less complicated fact, I shall attempt to explain it in the first place. Mr. Pictet, indeed, furnishes an explanation himself, by ascribing it to the evaporation of moisture from the ground. But to show that this is not just, it need only be mentioned, that the appearance never occurs in any considerable degree, except upon such

* Theophrastus also remarks, that it freezes less on hills than on plains, but without mentioning, that this happens only on calm and serene nights. Lib. v. c. xx.

nights as are attended with some dew, and that its great degrees are commonly attended with a copious formation of that fluid; since it cannot be thought, that the same stratum of air will deposit moisture on the ground, from an insufficiency of heat, at the very time it is receiving moisture from the ground, in the state of pellucid vapour, as this presupposes, that it is not yet replete with water.

Our atmosphere has been very generally regarded, as incapable of being heated directly by the rays of the sun, principally because these give no heat to any particular portion of it, in which they are brought to a focus. I do not know, whether this experiment was ever made with all the accuracy of which it is susceptible; but, granting that it has been thus made, my opinion is, notwithstanding, that no reliance can be placed in it. For as air, if heated at all by concentrated sunbeams, must be heated by them in a very slight degree, during the time that their focus may be looked upon as stationary, otherwise the present question would not have arisen, it is necessary for conducting the experiment properly, that, during the whole of it, the same individual small portion of air shall constantly receive that focus; but this, for various manifest reasons, cannot possibly happen. Viewing, therefore, the argument founded upon this experiment as without force, I shall now offer several considerations, which seem to prove, that air is actually heated by the sunbeams, which enter it.

1. Air both reflects and refracts light, and all other bodies, as far as I know, acquire heat, while they act thus on the light of the sun.

2. Air suffocates or absorbs the sun's light, which it cannot be supposed to do, without increasing in temperature.

3. If air, considered as an uniform fluid, were even incapable of gaining heat directly from the sun's rays, heat would be communicated by them to it, through the intervention of the innumerable particles of solid matter, which the trivial experiment of receiving a sunbeam into a darkened room shows to be present in the atmosphere. Should it be said, that this appearance may occur only in the neighbourhood of the earth, from the accidental admixture of solid matter raised from its surface by winds, or in any other way, the answer is, that, as my inquiry is concerning the existence of a certain condition of the atmosphere, it matters not how this originates. Nothing more can be demanded, than that it should always be found, which I believe to be the case; since, if I can trust my memory with respect to what took place many years ago, I should say, that such

particles are to be seen, by means of the sun's light, in the air over the middle of the Atlantic ocean. These particles then must receive heat from the sunbeams, which impinge upon them, and this they will communicate to the contiguous pellucid air.

4. Unless it be admitted, that the atmosphere is capable of intercepting part of the heat, which is radiated into it by the sun, and of converting this into heat of temperature, I deem it impossible to find a sufficient reason, for the great warmth which exists, after a long calm, in air incumbent upon the Atlantic and Pacific oceans, at the distance of a thousand miles or more from any considerable body of land. It cannot be derived from the neighbouring water, since this is colder than the lower atmosphere; and no one will suppose it to be the same heat, which the air had acquired from the last continent it had passed over, many days before. But, if even this were supposed, another difficulty would remain to be removed, which is, that, during the whole of the calm, the air is cooled every night, and again becomes warm in the day *.

* One reason is hence apparent for the great coldness of the high regions of the atmosphere; since the air in them must be less fit, than that of the lower strata, to arrest heat which is radiated into it.

Should what has been said be thought sufficient to establish, that the air arrests part of the sun's heat, which is radiated into it bound up with light, two consequences must also be allowed. The first is, that air will exert a greater power of the same kind upon heat radiated into it without light, since the sun's heat passes instantaneously through many bodies, which refuse a similar way to heat radiated by terrestrial substances; the other, that air must be as capable of becoming cold by radiating its own heat *, as of becoming warm from heat radiated into it, as these two properties are uniformly observed to exist together, and to be proportional to each other. The truth of the latter conclusion may also be inferred from this fact, that in still and calm weather the heat of the air, a few feet above the earth, will sometimes decrease, even in this country, 18 or 20 degrees between sunset and sunrise, though no change of wind has in the meantime occurred; for the inconsiderable conducting power, which air is now known to possess, will permit only a small part of this diminution to arise from heat passing, by means of that power, from the

* Mr. Prevost says: "On peut supposer que les molecules de l'air rayonnent." Du Calorique Rayonnant, p. 24.

atmosphere to the colder earth. Mr. Leslie *, indeed, ascribes this effect to the descent of cold air from the higher regions of the atmosphere; but if this were just, a less cold ought to be found, on a clear and still night, in the lower than in the higher strata, which is contrary to the uniform results of numerous experiments by Mr. Pictet and Mr. Six. Winds too, which produce such a mixture, always lessen the nocturnal decrease of temperature in the lowermost part of the atmosphere.

Having thus shown, that air is capable, both of absorbing heat, which is radiated into it, and of radiating heat, which had before formed a part of its temperature, I proceed to apply the knowledge of these facts, to the explanation of the phenomenon observed by Mr. Pictet and Mr. Six.

This phenomenon occurs on those nights only, which permit bodies, on the surface of the earth, to become cold by radiating their heat to the heavens. On other nights, when bodies, thus situated, were not colder than the air, I have observed the atmosphere, within the limits of 9 feet from the ground, the boundary of my own experiments, to decrease a little in

* On Heat and Moisture, p. 11, and 132.

temperature, as the distance from the earth increased. Mr. Six likewise found, that, on cloudy nights, the air was sometimes colder 220 feet above the ground, than at the distance of 9 feet from it. When, therefore, the earth has become colder, from radiation, than the neighbouring air, in consequence of the latter having, by reason of its small radiating power, emitted a less proportion of its heat to the heavens, the warmer air must radiate a part of its heat to the earth, without receiving a full compensation, and will therefore become colder, than it otherwise would have been. In proportion too as the air is nearer to the earth, must the cold of the former from this cause be the greater. My own conception of this matter is facilitated*, by contemplating the occurrence of an opposite effect, when the earth is warmer than the air. Let it be supposed then, that while the earth, in this state, radiates upwards a quantity of heat, a foot in depth of the incumbent air is capable of stopping a 1000th of what it hence receives, and of converting it into heat of temperature. The consequence must be that the next foot, from receiving only 999 parts of what had been emitted by the earth, will not be so much heated as the first

* The same facility is afforded by considering cold as a body.

foot, though it should absorb the same proportional quantity of what enters it. In this way, every successive foot will acquire a less quantity of heat than the preceding, and a state of the atmosphere be produced, like to that which is actually observed in a calm and sunny day. In the day, however, the phenomena, from the heating of air by rays from the earth, are somewhat confused by the warmed portions rising upwards, and mixing with what is colder; whereas, at night, the air, which has been cooled by radiating heat to the earth, is rendered, by an increase of gravity, the more fit to retain its low position. I have here, for the sake of simplifying the argument, taken no notice of the cooling of any considerable mass of the air, in consequence of the actual contact of its lowermost stratum with the earth, or by the conduction of the temperature of one portion of it to another. But, in a calm state of the atmosphere, these effects must be inconsiderable, though it appears to me impossible, in the present state of our knowledge, to determine them with any precision.

According to the view, which has been given by me of this subject, the heat of the air, in a clear and calm night, ought to increase, within the limits of the phenomenon, in some decreasing geometrical ratio, as the atmosphere ascends; and this conclusion is so far confirmed, by the observations of Mr. Pictet and Mr. Six taken together, that the increase of temperature is found to be greater in a given space very near to the earth, than in an equal space more remote from it.

To return to the immediate object of this article, the fact is certain, whatever may be thought of my explanation of it, that, in every clear and still night, the air near to the earth is colder than that which is more distant from it, to the height at least of 220 feet, this being the greatest to which Mr. Six's experiments relate. If then a hill be supposed to rise from a plain, to the height of 220 feet, having upon its summit a small flat surface covered with grass; and if the atmosphere, during a calm and serene night, be admitted to be 10° warmer there, than it is near the surface of the low ground, which is a less difference, according to the observations of Mr. Six, than what sometimes occurs in such circumstances, it is manifest, that, should both the grass upon the hill, and that upon the plain, acquire a cold of 10° by radiation, the former will, notwithstanding, be 10° warmer than the latter.

But the equality here supposed to be in the cold acquired by grass, in two such situations, can seldom exist. For, according to an observation made by Aristotle*, and since frequently repeated, the air of high places is much more agitated, than that upon low ground. The frequent renewal, therefore, from this cause, of the air in contact with the grass on the hill, will prevent it from ever becoming much colder than the general mass of the atmosphere, at the same height. Consequently, any diminution in this way of the 10° of cold, formerly supposed to occur there from radiation, must be added to the difference of temperature in the grass in the two situations.

What has hitherto been said refers only to the occurrences on the very summit of the hill. With respect to its sides, these can be only a little colder than the atmosphere upon a level with them, even in its calmest state. For, in the first place, they do not enjoy the full aspect of the sky; and, in the second, the air, which is cooled by contact with them, will, from its increased gravity, slide down their declivity, and thus make room for the application of new and warm parcels to the same surface. The motion too, thus excited in the air, near to the sides of the hill, must occasion a motion in that

* Meteor. lib. 1. c. x.

upon the summit, which may, in some measure, account for the last-mentioned observation of Aristotle, as far as relates to what happens in a clear night.

The height of the hill, in this example, has been supposed to be small, to make it accord with that of the stations, whose temperatures were compared by Mr. Six with the heat of the air near the ground. But observations of the same kind will apply to hills of much greater elevation. For granting, first, that the air at the height of 220 feet is never more than 10° colder, than that near to the earth, which is not probable, and is indeed contradicted by some of Mr. Six's observations; and again, that the increase of the air's heat, in a calm and serene night, ceases precisely at the greatest height, to which Mr. Six carried his observations, which is also improbable; still a reduction, to the extent of 10°, in the temperature of the air near to the earth, will render the cold of this low portion of the atmosphere greater than that of any other portion, which is not more than 2500 or 3000 feet above the former, if the estimate be just, which makes a declension in the heat of the atmosphere of 1° for every 250, or 300 feet of its height, when no counteracting cause exists.

The remarks, however, which have been

offered on the greater warmth of hills at night, in a certain state of weather, are strictly applicable to those only, which are insulated, and of inconsiderable lateral extent; and it is upon such chiefly, if not solely, that this phenomenon has been observed. The superiority of the cold of a low plain, from radiation, over that of a wide expanse of hilly ground, will, for obvious reasons, be less; and no superiority of this kind will probably exist in the former situation, when the high ground is not only extensive, but flat on the top, forming what is called a table-land; unless indeed, which seems to be actually the case, the air of such an elevated country should be commonly more agitated, than that of lower places equally level.

An explanation may be now easily given of an observation by Mr. Jefferson of Virginia *, which, however, had also been made by Aristotle †, and Plutarch ‡, that dew is much less copious on hills, than it is upon plains. For allowing, at first, the surface of the ground to be in both situations equally colder than the air which is near to it; still, as the production of dew must be in proportion to the whole depression of the temperature of the air which

* Notes on Virginia, p. 132. † Meteor. Lib. 1. c. x.

‡ De Primo Frigido.

furnishes it, below what its heat had been in the preceding day, and as one part of this depression, the general cooling of the atmosphere, is much more considerable on the plain than on the hill, moisture must necessarily be deposited more copiously in the former than in the latter place. If the greater agitation of the atmosphere, and the less quantity of moisture, during clear weather, in its higher region than in the lower, be added, it may readily be inferred, that dew shall sometimes be altogether wanting on a hill, though abundant on a plain at its foot, agreeably to what has been actually observed by Mr. Jefferson.

IX. The leaves of trees often remain dry throughout the night, while those of grass are covered with dew. As this is a similar fact to the smallness of dew on hills, I shall in accounting for it do little more, than enumerate the circumstances on which it depends.

1. The atmosphere is several degrees warmer near the upper parts of trees on dewy nights, than close to the ground. 2. The air in the higher situation is more agitated, than that in the lower. 3. The air at a little distance from the ground, from being nearer to one of its sources of moisture, will on a calm evening contain more of it, than that which surrounds the leaves of elevated trees. 4. Only the leaves

of the very tops of trees are fully exposed to the sky. 5. The declension of the leaves from an horizontal position will occasion the air, which has been cooled by them, to slide quickly away, and be succeeded by warmer parcels. 6. The length of the branches of the trees, the tenderness of their twigs, and the pliancy of the foot-stalks of their leaves, will cause in the leaves an almost perpetual motion, even in states of air that may be denominated calm. I have hence frequently heard, during the stillness of night, a rustling noise in the trees, which formed one of the boundaries of the ordinary place of my observations, while the air below seemed without motion.

Nearly in the same manner is to be explained, why shrubs and bushes also receive dew more readily than lofty trees.

X. Bright metals, exposed to a clear sky in a calm night, will be less dewed on their upper surface than other solid bodies; since of all bodies they will, in such a situation, lose the smallest quantity of heat by radiation to the heavens, at the same time that they are capable of receiving, by conduction, at least as much heat as any others from the atmosphere, and more than any others from the warmer solid substances, which they happen to touch.

If the exposed pieces of metal be not very small, another reason will contribute somewhat to their being later and less dewed than other solid substances. For, in consequence of their great conducting power, dew cannot form upon them, unless their whole mass be sufficiently cold to condense the watery vapour of the atmosphere; while the same fluid will appear on a bad conductor of heat, though the parts a very little beneath the surface are warmer than the air *.

From the same ready passage of heat from one part of a metal to another, a metallic plate suspended, horizontally, in the air several feet above the ground, will be found dewed on its lower side, if the upper has become so; while the lower surface of other bodies, more attractive of dew, but worse conductors of heat, are without dew in a similar situation.

A metal placed at night in the air, near to the ground, is, for the most part, sufficiently cold to condense, on its underside, the vapour which arises from the warmer earth; though

* I hence think it probable, that dew will sometimes form on the bulb of a thermometer, before the mercury in it is cooled below the temperature of the air. It seems certain to me, also, that dew may appear upon substances, which, from the thinness of the layer of matter their cold is confined to, will produce little or no sensible effect upon a thermometer that is applied to them.

its upper surface may be dry, from possessing the same, or almost the same temperature, as the atmosphere near to it.

As the temperature of metals is never much below that of the neighbouring air, a slight diminution of their cold from radiation will often occasion them to evaporate the dew, which they had previously acquired, though other substances, which had been more cooled by radiation, are still attracting dew. For a like reason, a metal, which has been purposely wetted, will often become dry at night, while other substances are becoming moist.

A substance highly attractive of dew, such as wool, if laid upon a metal, will derive heat from it, and will therefore acquire less dew, than an equal portion of the same substance laid upon grass.

A large metallic plate will be less readily dewed while lying on grass, than if it were placed in the air, though only a few inches above the grass; because, in the former situation, it receives freely, by means of its great conducting power, heat from the earth; whereas, when placed in the air, it powerfully resists by another property, possessed in a great degree by bright metals, the entrance of heat radiated towards it by the grass beneath. Besides; the grass under the metal possesses now less heat, than when this substance was in contact with it, partly from having a small oblique aspect of the sky, and partly from receiving air, which has been cooled by passing over other grass fully exposed to the heavens.

When a piece of metal, having closely applied to its under surface a substance of some thickness, which attracts dew powerfully, and, therefore, imbibes readily heat that is radiated to it, is exposed to the sky at night, the heat supplied by the attached substance, both from its own original store, and from what it has acquired through the radiation of the ground to it during the exposure, will enable this piece to resist longer, than a bare piece, the formation of dew, or even than another piece, which has only a thin coat of matter considerably attractive of dew attached to its underside. The experiment with the wooden cross, covered with gilt paper, affords an example of the latter fact.

A very small metallic plate, suspended in the air, is less readily dewed than a large one, similarly situated, as it receives, in proportion to its size, more heat from the atmosphere. On the other hand, a very small plate laid upon grass, rendered cold by radiation, will be sooner dewed than a larger one in the same situation, from presenting a greater proportional circumference to the surrounding grass, and therefore losing more quickly its heat by conduction. It will be also sooner dewed than another very small plate suspended in the air; since the latter, like other small bodies similarly placed, must be continually acquiring more heat than the former, in the manner described above in this Essay *.

A piece of metal, applied to different portions of cold grass in succession, will sooner become cold itself, than another piece, which is suffered to remain constantly upon one portion of the same grass, and will in consequence be sooner dewed.

If the bare side of a piece of metalled paper be exposed to a clear and calm sky at night, it will become cold, by radiation, and receive, by conduction, the heat of the inferior metallic surface; whence, if this surface be afterwards made the upper one, it will sooner acquire dew than a similar metallic surface, which has been exposed to the sky during the whole of the experiment.

When a metal covers, in part only, the upper surface of a piece of glass, the uncovered portion of the glass quickly becomes cold by radiation, on exposure to a serene sky in a still night, and then, by deriving to itself a part of the heat of the metal, occasions this body to be more readily dewed, than if the whole of the exposed

* Page 213.

surface had been metallic. In this experiment, the outer edge of the metallic surface, from being nearest to the colder glass, will be the first and the most dewed, while the parts of the uncovered glass, which are contiguous to the warmer metal, will be the last and the least dewed, of their respective substances.

A piece of glass, covered on one side with a metal, being placed on grass, with this side down, its upper surface attracts dew as readily as if no metal were attached to it; since the metal, in this situation, has no power to lessen the radiation of heat from the upper surface of the glass. I conclude, however, from general principles, for I have not made the trial, that if the same piece of glass, having its metallic side still undermost, were raised in the air a little above the grass, it would be more readily dewed on its upper surface, than if it had been without a metallic coating on the lower, as this coating must resist the introduction of heat radiated by the warmer grass, and thus preserve nearly undiminished the cold acquired, from radiation of heat to the sky, by the bare upper surface.

The preceding remarks apply to the whole class of metals; but the discoveries of Mr. Leslie, respecting the difference in the capacities of these bodies to radiate heat, furnish an explanation of a diversity among themselves, in regard to attraction for dew, which was noted in the foregoing part of this Essay. Gold, silver, copper and tin, are there said to resist the formation of dew more strongly, than other substances of the same class; but these metals, according to Mr. Leslie, radiate heat the most sparingly. On the other hand, lead, iron and steel, which, according to the same author, radiate heat more copiously than the former metals, were found by me to acquire dew more readily. I do not know, if the radiating power of platina has been ascertained by direct experiments; but, as its conducting power is small, its radiation must be great, since these qualities exist always in opposite degrees in the same substance; and I have accordingly observed it to be dewed, while the four first-mentioned metals were dry. I am ignorant both of the radiating and the conducting power of zinc, as determined by ordinary experiments; but I infer, from its being more easily dewed than gold or silver, that it radiates heat more copiously than they do; unless indeed, the pieces which I used, from having had their surfaces roughened by friction with sand, which was employed to brighten them, had acquired a radiating power, greater than that possessed by polished pieces, agreeably to the results of some of Mr. Leslie's experiments *.

* I once intended to subjoin here an explanation of some very curious observations by Mr. Benedict Prevost on dew,

XI. Thinking it probable, that black bodies might radiate more heat to the sky, at night, than white, I placed upon grass, on five different evenings, equal parcels of black and white wool. On four of the succeeding mornings, the black wool was found to have acquired a little more dew than the white; whence I inferred that it had, in consequence of its colour, radiated a little more heat. But I afterwards remarked, that the white wool was somewhat coarser than the black; which circumstance alone was

which were published, first in the 44th volume of the French Annals of Chemistry, and afterwards by Mr. Peter Prevost of Geneva, in his Essay on Radiant Heat; but fearing to be very tedious, I have since given up the design. I will say, however, that, if to what is now generally known on the different modes, in which heat is communicated from one body to another, be added the two following circumstances; that substances become colder, by radiation, than the air, before they attract dew; and that bright metals, when exposed to a clear sky at night, become colder than the air much less readily than other bodies; the whole of the appearances observed by Mr. Prevost may be easily accounted for.

Note to second edition.] I found, shortly after the publication of the former edition of this Essay, that the learned Dr. Young had, several years before, in his great work on Natural Philosophy, employed the principle of the radiation of heat to account for several of the facts observed by Mr. B. Prevost. On the subject of Dr. Young's explanation, I have spoken somewhat fully in the 28th number of Dr. Thomson's Annals of Philosophy.

sufficient to occasion a difference in their quantities of moisture. Another night, I laid on the raised board a piece of pasteboard covered with white paper, and close to this a second piece similar to the former in every respect, except that it was covered with paper blackened with ink. At daylight, I saw hoarfrost upon both pieces; but the black seemed to have a greater quantity than the white. A doubt, however, afterwards arose upon the accuracy of this experiment likewise; for, as the light was faint, when I viewed the two surfaces, the quantity of hoarfrost, though equal on both, might have appeared greater on the black, than on the white, from the contrast of its colour with that of the former surface. But trials of this kind, as Mr. Leslie* has observed, never afford firm conclusions; since a black body must always differ from a white in one or more chemical properties, and this difference may of itself be competent to produce a diversity in their powers to radiate heat.

With the view to render the subject less complicated, I have hitherto treated of dew, as if it

* On Heat, p. 95.

were altogether derived from watery vapour previously diffused through the atmosphere; this appearing to me to be by far its most considerable source, and none of my conclusions of any importance being liable to be affected, even by the establishment of a contrary opinion. Other writers, however, have regarded dew as being entirely the product of vapour emitted, during the night, by the earth and plants upon it. According to this theory, dew is said to *rise*.

The first trace, which I have found of the opinion, that dew rises from the earth at night, occurs in the History of the Academy of Sciences for 1687. It is mentioned there briefly and obscurely, and was, probably, shortly forgotten; for Gersten, who advanced it anew in 1733, held himself to be its author. Musschenbroek and Dufay embraced it immediately after Gersten; but the former soon admitted, that dew sometimes *falls*. As far as I have learned, no writer upon dew has since ascribed its total production to vapour, emitted by the earth at night, except Mr. Webster of New England*. But this opinion is frequently advanced in conversation by persons, not much accustomed to philosophical pursuits, chiefly, I think, because it contradicts a popular belief.

* Mem. of American Acad. vol. III.

The only argument used by the French academicians, in support of their opinion, is, if I understand it rightly, that as much dew is observed under an inverted glass-bell, as in any other situation. But admitting, for a moment, this to be true, they would not thus prove, that the ground is the only source of that fluid.

Gersten was led to think, that dew rises from the earth, by often finding grass, and low shrubs, moistened with it, while trees were dry. Respecting this fact, I shall add nothing to what I have lately said upon it. But his chief argument is derived from another fact related in the first part of this Essay, which is, that a plate of metal, laid upon bare earth on a dewy night, will remain dry on its upper surface, while it becomes moist on the lower. This also is easily explicable by what has already been mentioned by me. For the lower side of the metal, in consequence of the upper being in contact with the air and being exposed to a clear sky, is colder than the earth a little below the surface, and therefore condenses the vapour, which strikes against its bottom; while the upper side, from being frequently warmer, and never more than a little colder than the air, is for the most part unable to condense the watery vapour of the atmosphere*. Gersten, moreover, describes several appearances himself, which refute his opinion. He mentions, for example, that the higher parts of shrubs are more dewed than the lower; that metallic plates, placed horizontally in the air, are as much dewed on their superior, as on their inferior surfaces; and that convex and cylindrical bodies, suspended in the air, the latter having a position parallel to the horizon, are dewed only on their upper parts.

The principal reason given by Dufay for the rising of dew is, that it appears more early on bodies near to the earth, than on those which are at a greater height. But this fact readily admits of an explanation on other grounds, that have already been mentioned. 1. The lower air, on a clear and calm evening, is colder than the upper, and will, therefore, be sooner in a condition to deposit a part of its moisture. 2. It is less liable to agitation than the upper. 3. It contains more moisture than the upper, from receiving the last which has risen from the earth, in addition to what it had previously

* I have, in like manner, observed, on a cloudy night, a piece of glass, laid over an earthen pan containing water and placed upon the ground, to be wet on its lower side, while the upper was dry; the glass being, in this situation, sufficiently cold to condense the vapour of water heated by the earth, but not enough so to condense the watery vapour of the atmosphere.

possessed, in common with other parts of the atmosphere. Dufay attempted to strengthen his argument, by exposing, on three dewy nights, similar substances at different heights from the ground, expecting that the lower would always acquire more moisture than the upper; but, upon all the nights, some one of the lower substances acquired less moisture, than some one of the higher.

Mr. Webster has advanced no new fact in favour of the opinion, of which I am speaking.

Enough having been said to prove, that dew is not entirely the product of vapour rising from the earth at night, I shall next show, that it often occurs, when this cause can have little or no operation.

1. It appears from Hasselquist and Bruce, that in Egypt, shortly before the rising of the Nile, and consequently when the ground there is in its driest state, dew becomes exceedingly plentiful, though little or none had formed before, while the earth was somewhat less dry. The cause evidently is, as was formerly mentioned, the moist air brought from the Mediterranean by the north wind, which then prevails.

2. Mr. Webster, speaking of hoarfrost, which he properly regards as frozen dew, candidly says, though it overthrows his opinion: "This

frost appears, when the surface of the earth is sealed with frost, and of course the vapour of which it is formed, cannot at the time, perspire from the earth."

3. I have myself, at all seasons of the year, frequently observed wool, upon the middle of the raised board, and therefore out of the way of vapour rising from the ground, to acquire more dew, than wool laid upon the grassplat.

4. The bodies, that condense the rising vapour, must necessarily be colder than it; but, as they are likewise, according to the opinion under view, of the same temperature with the air surrounding them, this also should condense the rising vapour. Dew, therefore, should never appear in any considerable quantity, without being accompanied with fog or mist. Now I can assert after much attention to this point, that the formation of the most abundant dew is consistent with a pellucid state of the atmosphere. Hasselquist makes a similar observation, with regard to Egypt; where, during the season remarkable for the most profuse dews, "the nights," he says, "are as resplendent with stars, in the midst of summer, as the lightest and clearest winter nights in the north."

But, although these facts prove, that copious dews may occur with little or no contribution by vapour immediately rising from the earth, it must yet be admitted, that some of the moisture, which forms during clear and still weather, on bodies situated upon or near its surface, is in most cases to be attributed to this source; since, in my experiments, substances on the raised board became much later moist than others on the ground, though equally cold with them. The quantity from this cause, however, can never be great. For in the first place, until the air be cooled by the substances attractive of dew, with which it comes in contact, below its point of repletion with moisture, it will be always in a condition to take up that which has been deposited upon grass, or other low bodies, by warm vapour emitted by the earth; just as the moisture formed upon a mirror by our breath is, in temperate weather, almost immediately carried away by the surrounding air. Accordingly; I have sometimes, in serene and still weather, observed dew to appear sparingly upon grass in the shade, several hours before sunset, and to continue in nearly the same quantity till about sunset, when it would increase considerably, at the time that the same fluid began to show itself on the raised board. In the second place; though bodies situated on the ground, after they have been made sufficiently cold, by radiation, to condense the vapour of the atmosphere, will be able to retain the moisture, which they acquire by condensing the vapour of the earth; yet, before this happens, the rising vapour must have been greatly diminished, by the surface of the ground having become much colder. These considerations, added to the fact, that substances on the raised board attracted rather more dew, throughout the night, than similar substances lying on the grass, warrant me to conclude, that on nights, favourable to the production of dew, only a very small part of what occurs is owing to vapour rising from the earth; though I am acquainted with no means of determining the proportion of this part to the whole. On the other hand, however, in a cloudy night, all the dew that appears upon grass may sometimes be attributed to a condensation of the earth's vapour; since I have several times, in such nights, remarked the raised board to be dry, while the grass was moist. These nights were calm, and evaporation from the grass consequently not copious. When evaporation on cloudy nights was assisted by wind, dew has never, as was mentioned in the first Part of this Essay, been any where observed by me*.

* The interval between the first appearance of dew in the afternoon on grass, in shaded places, and sunset, was formerly said by me, on the authority, however, of only a few observations, to be considerably greater, than that between

Agreeably to another opinion, the dew found upon growing vegetables is the condensed vapour of the very plants, on which it appears. But this also seems to me erroneous for several reasons. 1. Dew forms as copiously upon dead as upon living vegetable substances. 2. The transpired humour of plants will be carried away by the air which passes over them, when they are not sufficiently cold to condense the watery vapour contained in it; unless, which is almost never the case if mist does not already exist, the general mass of the atmosphere be incapable of receiving moisture in a pellucid form. Accordingly, on cloudy nights, when the air, consequently, can never be cooled more than a little below the point of repletion with

sunrise, and the ceasing of the formation of dew upon grass in the morning. These observations were made on spots exposed during the greater part of the day to the sun. In such places, the heat acquired, from the sun, by the uppermost layer of earth, will be longer retained, than that acquired by the grass, which will, therefore, be sufficiently cool, soon after the heat of the day has declined, to condense a part of the vapour then copiously rising from the earth; whereas in the morning, both less vapour will rise, the surface of the earth having now lost a great part of its heat, and a less proportion of that which does rise will be condensed by the grass, as the temperature of this body now more nearly approaches that of the ground, from first receiving the heat of the sun reflected from the atmosphere and other substances.

moisture, by bodies in contact with it, dew is never observed upon any plants, that are elevated a few feet above the ground. 3. If a plant has become, by radiating its heat to the heavens, so cold, as to be enabled to bring the air in contact with it below the point of repletion with moisture, that which forms upon it, from its own transpiration, will not then, indeed, evaporate. But other moisture will, at the same time, be communicated to it by the atmosphere; and when the difference in the copiousness of these two sources is considered, it may, I think, be safely concluded, that almost the whole of the dew, which will afterwards form on the plant, must be derived from the air; more especially when the coldness of a clear night, and the general inactivity of plants in the absence of light, both lessening their transpiration, are taken into account.

An experiment, however, has been appealed to in proof, that the dew of plants actually does originate from fluid transpired by them; that namely, in which a plant, shut up in an air-tight case, becomes covered with moisture. But this experiment, if attentively examined, will be found to have little weight. First; the inclosed plant, being exempt from the cold, which its own radiation would have produced in its natural situation, on a dewy night, will transpire a greater quantity of fluid, than a similar plant exposed at the same time to the open air. Again; the small quantity of air, contained in the case, must soon be replete with moisture, after which, the whole of what is further emitted by the plant will necessarily assume the form of a fluid, whatever may be the condition of the external atmosphere; whereas, during even the clearest night, only a part of the smaller quantity of moisture, emitted by the exposed plant, will be condensed on its surface. In the last place; notwithstanding the circumstances, which favour the appearance of moisture upon inclosed plants from their own transpiration, still the quantity observed on them is said to be, for I have made no experiment myself respecting this matter, much less considerable, than what is seen upon plants of the same kind, exposed to the air for the same time, during a calm and serene night.

PART III.

OF SEVERAL APPEARANCES CONNECTED WITH DEW.

THERE are various occurrences in nature, which seem to me strictly allied to dew, though their relation to it be not always at first sight perceivable. The statement and explanation of several of these will form the concluding part of the present Essay.

I. I observed one morning, in winter, that the insides of the panes of glass in the windows of my bedchamber were all of them moist, but that those, which had been covered by an inside shutter, during the night, were much more so, than others which had been uncovered. Supposing, that this diversity of appearance depended upon a difference of temperature, I applied the naked bulbs of two delicate thermometers to a covered and uncovered pane; on which I found, that the former was 3° colder than the latter. The air of the chamber, though no fire was kept in it, was at this time $11\frac{1}{2}$°

warmer than that without. Similar experiments were made on many other mornings, the results of which were; that, when the warmth of the internal air exceeded that of the external, from 8° to 18°, the temperature of the covered panes would be from 1° to 5° less than that of the uncovered; that the covered were sometimes dewed, while the uncovered were dry; that at other times both were free from moisture; that the outsides of the covered and uncovered panes had similar differences with respect to heat, though not so great as those of the inner surfaces; and that no variation in the quantity of these differences was occasioned by the weather's being cloudy or fair, provided the heat of the internal air exceeded that of the external equally in both of those states of the atmosphere.

The remote reason of these differences did not immediately present itself. I soon, however, saw, that the closed shutter shielded the glass, which it covered, from the heat, that was radiated to the windows by the walls and furniture of the room, and thus kept it nearer to the temperature of the external air, than those parts could be, which, from being uncovered, received the heat emitted to them by the bodies just mentioned.

In making these experiments, I seldom

observed the inside of any pane to be more than a little damped, though it might be from 8° to 12° colder than the general mass of the air in the room; while, in the open air, I had often found a great dew to form on substances, only 3° or 4° colder than the atmosphere. This at first surprised me; but the cause now seems plain. The air of the chamber had once been a portion of the external atmosphere, and had afterwards been heated, when it could receive little accession to its original moisture. It consequently required being cooled considerably, before it was even brought back to its former nearness to repletion with water; whereas the whole external air is commonly, at night, nearly replete with moisture, and therefore readily precipitates dew, on bodies only a little colder than itself.

When the air of a room is warmer than the external atmosphere, the effect of an outside shutter, on the temperature of the glass of the window, will be directly opposite to what has been just stated; since it must prevent the radiation, into the atmosphere, of the heat of the chamber transmitted through the glass.

II. Count Rumford * appears to have rightly conjectured, that the inhabitants of certain hot

* Phil. Trans. 1804. p. 182.

countries, who sleep at nights on the tops of their houses, are cooled, during this exposure, by the radiation of their heat to the sky; or, according to his manner of expression, by receiving frigorific rays from the heavens. Another fact of this kind seems to be the greater chill, which we often experience upon passing, at night, from the cover of a house into the open air, than might have been expected from the cold of the external atmosphere. The cause, indeed, is said to be the quickness of transition from one situation to another. But, if this were the whole reason, an equal chill would be felt in the day, when the difference, in point of heat, between the internal and external air, was the same as at night, which is not the case. Besides; if I can trust my own observation, the feeling of cold from this cause is more remarkable in a clear than in a cloudy night, and in the country, than in towns. The following appears to be the manner, in which these things are chiefly to be explained.

During the day, our bodies while in the open air, although not immediately exposed to the sun's rays, are yet constantly deriving heat from them, by means of the reflection of the atmosphere. This heat, though it produces little change on the temperature of the air which it traverses, affords us some compensation for what we radiate to the heavens. At night also, if the sky be overcast, some compensation will be made to us, both in towns and in the country, though in a less degree than during the day, as the clouds will remit towards the earth no inconsiderable quantity of heat. But on a clear night, in an open part of the country, nothing almost can be returned to us from above, in place of the heat which we radiate upwards. In towns, however, some compensation will be afforded, even on the clearest nights, for the heat which we lose in the open air, by that which is radiated to us by the surrounding buildings.

To our loss of heat by radiation, at times that we derive little compensation from the radiation of other bodies, is probably to be attributed a great part of the hurtful effects of the night air. Descartes * says that these are not owing to dew, as was the common opinion of his cotemporaries, but to the descent of certain noxious vapours, which having been exhaled from the earth during the heat of the day, are afterwards condensed by the cold of a serene night. The effects in question certainly cannot be occasioned by dew, since that fluid

* Meteorolog. c. vi.

does not form upon a healthy human body, in temperate climates; but they may, notwithstanding, arise from the same cause, that produces dew on those substances, which do not, like the human body, possess the power of generating heat, for the supply of what they lose by radiation or any other means.

III. I had often, in the pride of half knowledge, smiled at the means frequently employed by gardeners, to protect tender plants from cold, as it appeared to me impossible, that a thin mat, or any such flimsy substance, could prevent them from attaining the temperature of the atmosphere, by which alone I thought them liable to be injured. But, when I had learned, that bodies on the surface of the earth become, during a still and serene night, colder than the atmosphere, by radiating their heat to the heavens, I perceived immediately a just reason for the practice, which I had before deemed useless. Being desirous, however, of acquiring some precise information on this subject, I fixed, perpendicularly, in the earth of a grassplat, 4 small sticks, and over their upper extremities, which were 6 inches above the grass, and formed the corners of a square, the sides of which were 2 feet long, drew tightly a very thin cambric handkerchief. In this disposition of things, therefore, nothing existed to prevent the free passage of air from the exposed grass, to that which was sheltered, except the 4 small sticks, and there was no substance to radiate heat downwards to the latter grass, except the cambric handkerchief. The temperature of the grass, which was thus shielded from the sky, was upon many nights afterwards examined by me, and was always found higher than that of neighbouring grass which was uncovered, if this was colder than the air. When the difference in temperature, between the air several feet above the ground and the unsheltered grass, did not exceed 5°, the sheltered grass was about as warm as the air. If that difference, however, exceeded 5°, the air was found to be somewhat warmer than the sheltered grass. Thus, upon one night, when fully exposed grass was 11° colder than the air, the latter was 3° warmer than the sheltered grass; and the same difference existed on another night, when the air was 14° warmer than the exposed grass. One reason for this difference, no doubt, was that the air, which passed from the exposed grass, by which it had been very much cooled, to that under the handkerchief, had deprived the latter of part of its heat; another, that the handkerchief, from being made colder than the atmosphere by the radiation of its upper surface to the heavens, would remit somewhat less heat to the grass beneath, than what it received from that substance. But still, as the sheltered grass, notwithstanding these drawbacks, was upon one night, as may be collected from the preceding relation, 8°, and upon another 11°, warmer than grass fully exposed to the sky, a sufficient reason was now obtained for the utility of a very slight shelter to plants, in averting or lessening injury from cold, on a still and serene night.

In the next place; in order to learn whether any difference would arise from placing the sheltering substance at a much greater distance from the ground, I had 4 slender posts driven perpendicularly into the soil of a grass field, and had them so disposed in other respects, that their upper ends were 6 feet above the surface, and formed the angular points of a square having sides 8 feet in length. Lastly; over the tops of the posts was thrown an old ship flag of a very loose texture. Concerning the experiments made by means of this arrangement of things, I shall only say, that they led to the conclusion, as far as the events of different nights could rightly be compared, that the higher shelter had the same efficacy with the lower, in preventing the occurrence of a cold upon the ground, in a clear night, greater than that of the atmosphere, provided the oblique aspect of the sky was equally excluded from the spots on which my thermometers were laid.

On the other hand; a difference in temperature, of some magnitude, was always observed on still and serene nights, between bodies sheltered from the sky by substances touching them, and similar bodies, which were sheltered by a substance a little above them. I found, for example, upon one night, that the warmth of grass, sheltered by a cambric handkerchief raised a few inches in the air, was 3° greater, than that of a neighbouring piece of grass, which was sheltered by a similar handkerchief actually in contact with it. On another night, the difference between the temperatures of two portions of grass, shielded in the same manner, as the two above-mentioned, from the influence of the sky, was 4°. Possibly, experience has long ago taught gardeners the superior advantage of defending tender vegetables, from the cold of clear and calm nights, by means of substances not directly touching them; though I do not recollect ever having seen any contrivance for keeping mats, or such like bodies, at a distance from the plants, which they were meant to protect.

Walls, I believe, as far as warmth is concerned, are regarded as useful, during a cold night, to the plants which touch them, or are near to them, only in two ways; first, by the mechanical shelter which they afford against cold winds, and secondly, by giving out the heat which they had acquired during the day. It appearing to me, however, that, on clear and calm nights, those on which plants frequently receive much injury from cold, walls must be beneficial in a third way, namely, by preventing, in part, the loss of heat, which they would sustain from radiation, if they were fully exposed to the sky, the following experiment was made for the purpose of determining the justness of this opinion.

A cambric handkerchief having been placed, by means of two upright sticks, perpendicularly to a grassplat, and at right angles to the course of the air, a thermometer was laid upon the grass close to the lower edge of the handkerchief, on its windward side. The thermometer thus situated was several nights compared with another lying on the same grassplat, but on a part of it fully exposed to the sky. On two of these nights, the air being clear and calm, the grass close to the handkerchief was found to be 4° warmer, than the fully exposed grass. On a third, the difference was 6°. An analogous fact is mentioned by Gersten, who says, that an horizontal surface is more abundantly dewed, than one which is perpendicular to the ground.

IV. The covering of snow, which countries in high latitudes enjoy during the winter, has been very commonly thought to be beneficial to vegetable substances on the surface of the earth, as far as their temperature is concerned, solely by protecting them from the cold of the atmosphere. But were this supposition just, the advantage of the covering would be greatly circumscribed; since the upper parts of trees and of tall shrubs are still exposed to the influence of the air. Another reason, however, is furnished for its usefulness, by what has been said in this Essay; which is, that it prevents the occurrence of the cold, which bodies on the earth acquire, in addition to that of the atmosphere, by the radiation of their heat to the heavens during still and clear nights. The cause, indeed, of this additional cold, does not constantly operate; but its presence, during only a few hours, might effectually destroy plants, which now pass unhurt through the winter. Again; as things are, while low vegetable productions are prevented, by their covering of snow, from becoming colder than the atmosphere in consequence of their own radiation, the parts of trees and tall shrubs, which rise above the snow, are little affected by cold from this cause. For their outermost twigs, now that they are destitute of leaves, are much smaller than the thermometers suspended by me in the air, which in this situation very seldom became more than 2° colder than the atmosphere. The larger branches too, which, if fully exposed to the sky, would become colder than the extreme parts, are, in a great degree, sheltered by them; and, in the last place, the trunks are sheltered both by the smaller and the larger parts, not to mention that the trunks must derive heat, by conduction through the roots, from the earth kept warm by the snow *.

In a similar way is partly to be explained the manner, in which a layer of earth or straw preserves vegetable matters in our own fields, from the injurious effects of cold in winter.

V. The bare mention of the subject of this article will be apt to excite ridicule, it being an attempt to show, in what way the exposure of animal substances to the moon's light promotes their putrefaction. I have no certain knowledge,

* It may be remarked here, however, that a thick covering of snow, while it renders the surface of the earth warmer that it would otherwise be, must occasion the lower atmosphere to be colder, by preventing the passage of the heat of the ground to the air, either by radiation or conduction.

that such an opinion prevails any where, at present, except in the West Indies; but I conclude, from various circumstances, that it exists also in Africa, and that it was carried thence by negro slaves to America. It was entertained, however, by persons of considerable rank and intelligence among the ancients; for Pliny * affirms it to be true, and Plutarch, after making it a subject of discussion in one of his Symposia †, admits it to be well founded.

As moonbeams communicate no sensible heat to the bodies, on which they fall, it seems impossible, that they can directly promote putrefaction. But still a reason, for ascribing such a power to them, may be derived from their being received by animal substances, at the very time that a real, but generally unnoticed, cause of putrefaction, in warm climates, (and it is in these alone the opinion I am treating of has ever prevailed) is taking place, which ceases to act, as soon as the moon's light is excluded.

The nights, on which a steady moonshine occurs, must necessarily be clear; and nights, which are clear, are almost always calm ‡. A

* Lib. ii. §. civ. † Lib. iii. Prob. x.

‡ Mr. De Luc has remarked, that clouds frequently disappear soon after sunset. *Idées sur la Meteorologie*, II. 98. I have often observed this myself, and at the same time another fact of which he takes no notice; namely, that the

moonshiny night, therefore, is one, on which dew forms plentifully; hence the expressions 'roscida' and 'rorifera luna' employed by Virgil and Statius; and hence also an opinion, held, as appears from Plutarch, even by philosophers among the ancients, that the moon communicates moisture to the bodies, which are exposed to its light *.

Animal substances are among those, which acquire dew in the greatest quantity. To do this, indeed, they must previously become colder than the atmosphere; but, having acquired the moisture of dew, in addition to their own, they will, on the following day, be in that condition, which is known, by experience, to favour putrefaction most powerfully in hot climates.

The immediate cause assigned here, for the quick putrefaction of animal substances, which have been exposed to the moon's rays in a hot country, is the same as that given by Pliny and Plutarch; but they attributed the origin of this

atmosphere is then calmer than it had been before sunset. This calmness of the air very commonly, if not always, precedes the dissipation of the clouds.

* Akin to this opinion of the ancients respecting the humefying quality of the moon, is one, which has been held, by modern writers as well as ancient, upon that planet's being a cause of cold to the bodies, which receive its rays; though I know of no author who has taken notice of this affinity.

immediate cause, the additional moisture, to the peculiar humefying quality, which they supposed that luminary to possess. This false theory has, probably, contributed to discredit, with the moderns, the circumstance which it was employed to explain.

VI. The last fact, of which I shall treat in this Essay, is the formation of ice, during the night in Bengal, while the temperature of the air is above 32°.

I have seen only two original descriptions of this process, both of which are contained in the Philosophical Transactions; the first, by Sir Robert Barker, in the 65th volume; the other in the 83rd, by Mr. Williams.

According to the method followed by Sir R. Barker's ice-maker, square excavations, 2 feet deep, and 30 wide, having been formed in a large open plain, their bottoms are covered with sugar-cane, or stems of Indian corn, dried, to the thickness of 8 inches or 1 foot. On this layer, are afterwards placed, in rows, near to each other, *small*, unglazed earthen pans, $\frac{1}{4}$ of an inch thick, and 1 inch and $\frac{1}{4}$ deep, filled with *boiled soft* water. The pans are sufficiently porous to allow their outer surface to appear moist, after water has been poured into them. Sir R. Barker adds; that the nights, the most favourable for the production of ice, are those, which are the calmest and most serene, and on which very little dew appears after midnight; that clouds and frequent changes of wind, are certain preventives of its formation; and that, although ice is thus very readily procured by art in Bengal, during the winter, it scarcely ever occurs there naturally.

The process described by Mr. Williams must, from its extent, 300 persons being employed in it, have been carried on for profit, and would, consequently, be conducted in the most economical manner. A piece of ground, nearly level, containing about 4 acres, was divided into square plats, from 4 to 5 feet wide, which were surrounded by little mounds of earth, 4 inches high. In these inclosures, previously filled with dry straw, or sugar-cane haum, were placed as many *broad*, shallow, unglazed earthen pans, containing *unboiled pump* water, as they could hold. The air was generally very still, when much ice was formed; wind prevented its formation altogether. In the morning, between 5 and 6 h., at which time alone, Mr. Williams made his observations, a thermometer, with its bulb naked, placed on the straw, amidst the freezing vessels, was never found by him lower than 35°; and he has observed ice, when a thermometer so placed was 42°. Another thermometer, suspended 5½ feet above the ground, was *commonly* 4° higher than that among the pans. It is possible, therefore, that Mr. Williams may have seen ice, a little before sunrise, when the temperature of the air was 46°. But granting this were the fact, it would not hence follow, that the ice was formed, while the air possessed that heat. For, although the air is generally held to be in all countries colder about sunrise, than at any other time, I know from my own observations, that this is not *always* the case in England; and similar exceptions may occur in Bengal. Sir H. Davy has said, in his Elements of Chemistry, that ice will form in Bengal, when the temperature of the air is not below 50°; but he has given no authority for this assertion.

The formation of ice, in the circumstances which have been just mentioned, was attributed by Sir R. Barker altogether, and by Mr. Williams in great measure, to cold produced by evaporation. Sir R. Barker's opinion has since been adopted by some of our most distinguished writers on Natural Philosophy, as Watson, Thompson, Young, Davy and Leslie, apparently, however, without their having fully considered it, as I shall now attempt to show.

1. It is necessary for the complete success of the process, that the air should be very still; wind, which so greatly promotes evaporation,

prevents the freezing altogether. Sir R. Barker admits, that the excavations in the earth are made to increase the stillness of the air in contact with the water in the pans; but, with the view to explain the utility of this stillness he supposes, in opposition to all experience, that water kept very quiet freezes more readily, when other circumstances are the same, than if it were a little agitated.

2. No proof is given, that evaporation from the pans actually does occur, at the times which are the most favourable for the appearance of ice. At any rate it cannot be considerable; since, agreeably to what is mentioned by Sir R. Barker, dew forms in a greater or less degree during the whole of the nights, the most productive of ice; and it is not to be thought, as was said upon a former occasion, that one portion of air will be depositing moisture, from possessing a superabundance of it, while another in the immediate vicinity is receiving moisture in great quantity, in the state of pellucid vapour; as the latter fact can exist only when the air is far removed from a state of repletion with water.

3. If evaporation produced the cold under consideration, the wetting of the straw or other matter, upon which the pans are placed, would tend to increase it; and, accordingly, Sir H.

Davy affirms this to be the case. But Mr. Williams, who must here be regarded as the better authority, says, that it is *necessary* to the success of the process that the straw be dry; in proof of which he mentions, that when the straw becomes wet, by accident, it is replaced; and that when he purposely wetted it in some of the inclosures, the formation of ice there was always prevented. The reasons are clear. The water, by softening the straw, renders it easily compressible by the weight of the pans, and at the same time fills up what would otherwise be vacant spaces among its parts. The straw, therefore, in this condensed state, must afford a ready passage to heat from the earth to the pans, the hindrance of which is allowed by every person to be the use of it, in this process, when dry. Again; the moisture, which passes through the straw to the earth it covers, will rise afterwards in the form of vapour, having the same temperature with the warm ground, and will communicate heat to the pans. In the last place; a part of this vapour will be condensed into water by the pans, in consequence of which heat must be extricated.

4. It is mentioned both by Sir R. Barker and Mr. Williams, in support of their opinions, that the pans, when new, are so porous, that they readily permit water to transude them; and

that old pans, which permit this in a less degree, are less fit for the making of ice. But the argument, which is hence derived by them, is completely refuted by a fact related by Mr. Williams himself; for he says, that the pans are greased before they are used, to prevent the adhesion of the ice to their sides; since, if this purpose be answered, the water can never be in contact with the pans, and therefore can never pass through them.

The real reason of the less fitness of old pans for the making of ice is perhaps the following. The production of the cold, which occurs in this process, must take place in the water; since neither the straw upon which the pans are placed, nor the air above them, was ever found by Mr. Williams of so low a temperature as 32°. Whatever, therefore, obstructs the passage of heat from the straw to the water, must favour the freezing of the latter. But this will be less effectually done by an old than by a new pan, as the density of the former is greater, from the grease forced into it by rubbing, and from the slime and sand that will enter with the water into its pores, when these are not entirely closed by the grease; which must often happen, as the smearing is performed only once in three or four days. The difference, however, in effect betwixt old and new pans must be very small; as it does not appear that the old are ever laid aside on account of their unfitness.

In a like way may be explained, without the aid of cold produced by the evaporation of moisture from the outsides of the pans, another fact mentioned by Mr. Williams, that ice was often found by him in those vessels, while water contained in a china plate, surrounded by them, had none; since the thin and dense substance of the plate must have transmitted more readily, than the thick and rare substance of the pans, the heat of the straw to the water.

5. In accounting for the making of ice in Bengal, it is requisite to show, not only how the first film is produced, but also, in what way the thickness of this film is afterwards increased. If evaporation be the cause of this increase, it follows, that a plate of ice in the night-time, and in the stillest air, both unfavourable to that process, must yet emit as much moisture, as is necessary for the production of a cold, according to Mr. Williams, of at least 14°, and according to Sir H. Davy of at least 18°; a conclusion, as it appears to me, of itself sufficient to destroy the credit of the theory, from which it is drawn.

While attending to this subject, I became desirous of acquiring some knowledge of the degree of cold, which might be produced by evaporation from water contained in a shallow vessel. With this view, I placed on a feather-bed, situated between the door and window of a room in my house in London, two china plates, into one of which as much water was poured, as covered its bottom to the depth of $\frac{1}{4}$ of an inch. The other plate was kept dry. The bulb of a small thermometer being then applied to the inside of the bottom of each plate, I observed upon many days, in various seasons of the year, the difference between these instruments while the door and window were open. I found, in consequence, that when the temperature of the air in the room was 75°, the highest at which any experiment was made, the thermometer in the plate, containing water, was between 6 and 7 degrees lower than the one in the dry plate; that the difference between these thermometers diminished gradually as the air became colder; and that when the temperature of the air was 40°, the lowest for which I have any observation, the difference was only $1\frac{1}{2}$°. At 32°, therefore, it would have been very small, and at a few degrees below 32 it would probably have vanished. This supposition agrees with an observation made by Mr. Wilson of Glasgow, who found, that no cold was produced by evaporation from snow possessing a temperature of 27°, though the air in the immediate neighbourhood was purposely much agitated by him.

The conclusions here given by me, respecting the cold produced by the evaporation of water, were drawn from experiments made in the day, while the sky was clear, the air very calm, and the temperature of the atmosphere stationary. At night, and during a cloudy day, the differences were less. On the other hand, if there was any perceptible motion in the air, they were greater. They were also greater if the heat of the atmosphere was increasing; but less, if this was decreasing.

Having thus, I think, placed beyond doubt, that the formation of ice in Bengal is not occasioned by evaporation, I shall now state several reasons, which have induced me to believe, that it depends upon the radiation of heat to the heavens.

1. This cause not only exists, but exists in a degree, sufficient for the production of the effect, which I attribute to it. For Mr. Wilson found the surface of snow, during a clear and calm night, to be 16° colder than air 2 feet above it, the temperature of the latter being taken by a naked thermometer; whereas the greatest heat of the atmosphere ever observed by Mr. Williams, at the distance of 5½ feet from the ground,

during the time that he supposed ice to be forming, was only 14° higher than the freezing point of water. I need say nothing of the difference of 18° related by Sir H. Davy, as he does not speak from his own observation, and as he gives no authority for what he advances; though even this difference is considerably less, than what I have attempted to show must sometimes occur, from the radiation of heat at night, between the temperature of air, a few feet above the earth, and that of bodies placed on its surface.

It is to be mentioned here also, that, according to Mr. Leslie*, the power of water to radiate heat, exceeds, perhaps, that of all other substances.

2. Ice is chiefly formed in Bengal during the clearest and calmest nights; and it is on such nights that the greatest cold, from radiation, is observed on the surface of the earth. In Sir R. Barker's more refined mode of conducting the process, an unusual stillness of the air, in contact with the water to be frozen, is procured, by placing the pans containing it a little below the level of the ground; in which situation, it was formerly shown, bodies must grow colder from radiation to the heavens at night, than in any other.

* On Heat, p. 80.

3. The cold, by means of which ice is produced in Bengal, appears, as I think may be inferred from what is said by Sir R. Barker, in its greatest degree, like cold from radiation in other substances, on those still and serene nights, during which little dew is deposited by the atmosphere.

4. Clouds and wind prevent the formation of ice in Bengal; and the same states of the atmosphere either prevent, or considerably diminish, the occurrence of cold from the radiation of heat at night by bodies on the ground.

I shall close this subject, by giving some account of a few attempts to procure the freezing of water at night, in this country, by exposing it to air of a temperature, higher than that of 32°. These were made by me in 1812, at my usual place of experiment, which was formerly stated to be not well adapted for the appearance of a great cold from radiation, and on nights not among the most favourable to such an undertaking, even of those which occur in this country. It is proper also to mention, that I was then less able to conduct such experiments, and to make use of them, than I afterwards became, from a longer attention to similar objects.

EXPERIMENT 1st.

With a view to imitate the method of making ice described by Sir R. Barker, I had a pit dug, on the evening of the 3rd of May, in the middle of the garden so often spoken of, 4½ feet long, 3 wide and 2 deep. It consequently had the same depth as the excavations mentioned by that gentleman, but was considerably less in its other dimensions. Clean dry straw was then strewed, to the height of a foot, over the bottom of the pit. On the straw were next laid a number of small shallow earthen pans, a part of which were glazed, and a part unglazed. In the last place; all the pans were filled with soft water, which had been boiled on the same evening. Contrary to my expectation, the unglazed pans remained as dry on the outside, after water had been poured into them, as those which were glazed. I conclude, therefore, that the former were more dense in their substance, than the unglazed pans used in India; and that their density was probably the reason, why ice did not afterwards form in them, sooner than in the glazed pans, which were employed by me.

Two pans, containing boiled water, were set upon the grassplat, at a little distance from the pit. A watch-glass filled with boiled water was also placed upon the grassplat, and another was laid upon the raised board, which had been thinly covered with sand. All these arrangements were not completed before 10h. at night.

At 1 h. in the morning, ice appeared in the watch-glasses on the grassplat and raised board; the heat of the air, as measured by a naked thermometer, being then, at 4 feet above the ground, $39\frac{1}{3}°$, and at 7 feet, $40\frac{1}{2}°$. At 2 h. ice was observed in the pans in the pit, while a thermometer in the air, $2\frac{1}{2}$ feet above the ground, was $36\frac{1}{2}°$. Shortly afterwards, ice began also to form in the pans upon the grassplat. The temperature of grass, fully exposed to the sky, was at the same time 30°, while that of the earth an inch below the bottom of the grass was 45°. During the time of these observations dew formed copiously.

EXPERIMENT 2nd.

My next attempt was in the manner mentioned by Mr. Williams.

On the evening of the 22nd of May, I encompassed a square piece of level ground, the sides of which were 3 feet long, with a border of earth 4 inches high, and filled the area with dry straw. On this were placed several of the earthen pans, which had been formerly used, and a few smaller vessels, all containing unboiled water. After an exposure of little more than an hour, water in a watch-glass upon the straw was found frozen, the temperature of the air, 2 feet above the straw, being then 37°. In half an hour more, ice began to appear in the earthen pans, while a thermometer 5½ feet above them, this being the height at which Mr. Williams used to suspend his instrument, was 36°. The air soon after became colder; but its temperature was never less than 33°, though taken by a naked thermometer, which, as was before said, upon a clear and calm night, occasions the air to seem about 2° colder than it really is.

It might be inferred, from what is mentioned by Mr. Williams, that the temperature of the straw beds, on which the ice-pans were set at Benares, was always found by him above the freezing point, for this reason, that the straw, from containing no moisture, could not, like the water, grow cold by evaporation. I had, therefore, been surprised, during the first experiment, for I had then but little acquaintance with the phenomena of cold observed with dew, that a thermometer, laid upon an exposed part of the straw, was always below the freezing point, after ice had begun to form in the pans. On reading, however, his account of the process a second time, with increased attention, my wonder ceased. For, as the pans he speaks of were *large*, and touched one another, and as all the pans employed in India, for the making of ice, widen as they rise from the bottom, like our milk-pans, the thermometer, placed by him on the straw, must have been secluded from all view of the sky, and would therefore mark a temperature much higher, than if it had been laid, as in my experiment, upon straw fully exposed to the heavens. On this, the second night, therefore, I placed a thermometer under the edge of one of the pans lying on the straw bed, and found it some time afterwards 6° higher, than a similar instrument upon a part of the straw bed which was uncovered. Generally, however, the difference was not so great. If my pans had been large, like those of Mr. Williams, I should, no doubt, have observed more considerable differences; for, in consequence of their smallness, I could not lay a thermometer on the straw bed, so as to be fully screened from the sky by the edge of any of them, without its being almost in contact with the vessel, every part of which was always colder than the sheltered straw.

Much dew formed in the course of this night. The greatest difference remarked by me, during it, between the temperatures of grass and of air, was 6°, and between those of air and a fully exposed part of the straw bed 9°.

EXPERIMENT 3rd.

This was begun on the evening of the 16th of October, and was likewise made agreeably to the method related by Mr. Williams.

Ice appeared in the pans, when the temperature of the air, at the height of 5½ feet, was, according to a naked thermometer, 37°.

On this night, I placed upon the straw bed a dry earthen pan, among those which contained water, and found the inside of its bottom to be as much colder than the air, as the water was in the other pans, before ice appeared in them. After the water had begun to freeze, no proper comparison could be made between its temperature and that of the empty pan. This pan, in the course of the night, attracted moisture, which was afterwards converted into a film of ice.

But the chief fact established by the present experiment was, that water may freeze at night, in air of a temperature higher than 32°, not only without any loss of weight from evaporation, but with a gain of weight from an opposite process.

I had observed that water, exposed early in the evening in the open air to the sky, lost a little weight, in the course of a clear night. This I imputed to evaporation taking place, before the water had been cooled enough to condense the vapour of the atmosphere, and to the weight gained afterwards being insufficient to compensate the previous loss. I exposed, therefore, on this night, water to the influence of the sky, until it was cooled to 34°. Of this I put 2 ounces into each of two china saucers, which had also been exposed to the air, and then placed the saucers upon the straw bed. In the morning, a thin cake of ice was found in both saucers, one of which had gained 2½, and the other 3 grains, in weight. Dew was also copious on this night. At one time, grass was 9½°, and the exposed part of the straw bed 12°, colder than the air*.

It must be evident to every person, that the formation of ice, in the three preceding experiments, was the effect of a natural operation, similar to that by which the same substance is produced in Bengal. These two facts must, therefore, have a common cause, and this has

* The greater cold, observed in this and the preceding experiment, upon straw than upon grass, is to be referred to the shortness of the latter, by reason of which heat was readily communicated to its upper parts by the earth.

been shown, by the last experiment, independently of what was said before in this Essay, not to be evaporation. It is also clear, that the cold, induced on the water in those experiments, had a common cause with that observed, at the same time, upon the grass and the straw; which latter cold must, in consequence of proofs formerly given, be admitted to have arisen from the radiation of the heat of those substances to the heavens. A necessary inference, therefore, appears to be, that the formation of ice in Bengal, in the circumstances described by Sir R. Barker and Mr. Williams, must likewise be attributed, in by far the greater measure, if not altogether, to a loss of heat, which the water suffers by its own radiation, while situated in such a manner, that it can receive little heat from other bodies, either by radiation or conduction*.

* On the evenings preceding the nights, during which ice is produced in Bengal, the temperature of the water exposed in the pans is, probably, often 60° or more. But water of the heat of 60°, if exposed in a shallow earthen vessel to air of the same temperature, during the day, while the weather is calm and clear, will lose about 3° of heat by evaporation. A cold from this cause may, therefore, concur with that from radiation, and, consequently, may, in Bengal, accelerate somewhat the formation of ice. The influence, however, of evaporation there, in this respect, should the state of the air

with regard to moisture still permit it, which must often not be the case while dew is forming, will, as the night proceeds, gradually diminish, and at length almost disappear, before the freezing of the water commences; since I have lately shown, that evaporation from water of 32° produces very little cold, even in the day-time. Indeed, it seems to me much more probable, that on a clear and calm night, though in a dry winter of Bengal, water at the temperature of 32° will acquire warmth from the formation of dew upon it, than that it will become cold from evaporation.

CONCLUSION.

The experiments which were made by me on dew, and other subjects treated of in the preceding Essay, were unavoidably attended with many inconveniences, which were the more felt, as my health had long been feeble, and as my professional duties obliged me often to return to London in the morning, without having previously taken rest, after the whole of a night had been spent in attending to the objects of my pursuit. The inconveniences here alluded to were, indeed, so great, that I was twice or thrice obliged to intermit my labours for several months together, and at length found it necessary to cease from them entirely, before I had nearly completed the plan, which I had formed. I take the liberty of mentioning these things, to excuse, in part, the imperfections, which will be observed in what I have written, as some of them would, no doubt, have been removed by a further interrogation of Nature*.

London, September 25, 1815.

* Of the experiments related in the beginning of the second Part of this Essay, with the view of proving, that the formation of dew is an effect of previous cold in the substances on

which it appears, those of only one evening were remarkable for the greatness of their results, the weather upon the other evenings not having favoured much my purpose. I took advantage, therefore, of being in the country, at the distance of a few miles from London, on the 21st of the present month, the last day but one of an unusually long tract of dry weather, to expose to the sky, 28 minutes before sunset, weighed parcels of wool and swandown, upon a smooth, unpainted, and perfectly dry fir table, 5 feet long, 3 broad, and nearly 3 in height, which had been placed an hour before, in the sunshine, in a large level grass-field. At this time, and throughout my experiments, the air was very still, and the sky very serene. The atmosphere, too, in all probability, contained but little moisture, in consequence of the long absence of rain; and the surface of the ground apparently contained none. The wool, 12 minutes after sunset, was found to be 14° colder than the air, the temperature of the latter being measured by a naked thermometer suspended 4 feet above the ground, and to have acquired no weight. The swandown, the quantity of which was much greater than that of the wool, was at the same time 13° colder than the air, and was also without any additional weight. In 20 minutes more, the swandown was $14\frac{1}{2}°$ colder than the neighbouring air, and was still without any increase of its weight. My experiments now ceased from a failure of daylight.

In my former experiments of this kind, the greatest cold observed by me from radiation, without the appearance of dew, was only $9\frac{1}{2}°$.

While making the experiments on wool and swandown, I attended frequently to the temperature of the grass, and found it at one time 15° colder than that of the air 4 feet above the ground. This difference is 1° greater, than any I had ever before seen between the temperatures of the same substances, and is equal to the greatest which I had ever known to occur, between those of the atmosphere and of swandown lying upon grass. I had this evening placed no swandown upon grass.

These experiments were not made till nearly the whole of the present edition of my Essay was printed, and could not, therefore, be mentioned in their proper place.

A

LETTER

TO

THE RIGHT HONOURABLE

LLOYD, LORD KENYON,

RELATIVE TO SOME CONDUCT OF THE

COLLEGE OF PHYSICIANS OF LONDON,

POSTERIOR TO THE

DECISION OF THE COURT OF KING'S BENCH

IN THE

CASE OF DR. STANGER;

AND CONTAINING

OBSERVATIONS ON A PRINCIPAL GROUND OF THAT

DECISION.

TO

DAVID HUME, ESQ.

ADVOCATE, PROFESSOR OF SCOTCH LAW IN THE UNIVERSITY OF EDINBURGH, &c.

MY DEAR FRIEND,

When you requested some months ago, that I would proceed no farther in the letter, which I had informed you I was writing to the Chief Justice of the Court of King's Bench, it appeared to me that you had, in a great measure, mistaken its object. I therefore considered myself entitled to continue my undertaking, and have accordingly now brought it to a conclusion. I readily admit, that, in one point of view, I may have been imprudent;

Ἄφρων δ' ὅς κ' ἐθέλοι πρὸς κρέισσονας ἀντιφερίζειν·
Νίκης τε στέρεται, πρὸς τ' ἄισχεσιν ἄλγεα πάσχει.

But I trust that, as far as the more important parts of moral character are concerned, you will find nothing in what I have done unworthy of the friendship between us, which, from its commencement, now nearly thirty years ago, when our boyish fancies gilded every prospect before us, has been ever my pride, and often, in the storms of life, the chief anchor of my hope.

As a piece of composition, my letter will no doubt be deemed faulty by you in many respects. You will perceive, for instance, a considerable want of unity in the execution, should indeed the great rules of criticism be thought applicable to such a trifle as the present, from the introduction of circumstances, which must seem both trifling and irrelevant, if the rank and character of the person to whom they are communicated be considered. My excuse for part of them is, that, it being one of my intentions to give information to some of my own profession, I conceived it allowable to mention various things for this purpose alone.

I shall anticipate only one other of your observations regarding my letter, and this refers to the lateness of its appearance. I confess myself much ashamed, that almost a twelvemonth has passed away since the occurrence of the event, which especially gave rise to it. But accidents, which would appear ridiculous in narration, whatever their effects may have been, often interrupted my labour, and indolence often pleaded, for a time, irresistibly against the performance of an ungrateful task, which duty had imposed. The delay, however, has necessarily tended to diminish the probability of there being many considerable errors in what I have advanced.

Forgive me for employing this mode of conveying my sentiments to you, and accept my warmest wishes for your welfare.

I remain,

Your most affectionate friend,

WILLIAM CHARLES WELLS.

London,
September 1, 1799.

LETTER

TO

THE RIGHT HONOURABLE

LLOYD, LORD KENYON, &c.

La fede unqua non deve esser corrotta,
O data a un solo, o data insieme a mille;
E cosi in una selva, in una grotta,
Lontan dalle cittadi, et dalle ville;
Come dinanzi a tribunali, in frotta
Di testimon, di scritti, e di postille;
Senza giurare, o segno altro piu expresso,
Basti una volta, che s'abbia promesso.

MY LORD,

IF confidence can be placed in the accuracy of the well-known writer of short-hand, Mr. Gurney, the decision of your Lordship, and the other Judges of the Court of King's Bench, in the case of Dr. Stanger, rested principally upon this ground—that he might readily obtain by a direct application to the College of Physicians, what he then prayed the court to enjoin that body to grant.—Every person, your Lordship said, has already a right to address himself to the honourable feelings of those breasts, to which Dr. Stanger must at last address himself, if the mandamus were issued. The same sentiment was immediately after expressed by you a second time; "if any one proposes him"—I venture to repeat your Lordship's words—"the question is submitted to a majority. It goes then to that tribunal, which, I hope and believe, is *the sanctuary of honour and good faith*, and he may as well address himself to them now, as if this mandamus went." I am, my Lord, one of those persons, whom you thus declared to have a right to address themselves to the honourable feelings of the College of Physicians of London. I have exercised that right.—I have applied to *the sanctuary of honour and good faith*, for a completion of those assurances, which your Lordship regarded so deeply imbedded in truth, that you erected upon them a decision, which was to affect the reputation and fortunes of many of your fellow-subjects, of no mean rank in society, and from which there could be no appeal. Of the success of this application I now think it my duty to inform you, as it originated in your counsel. The counsel was given in open court; the narration of its consequences ought, therefore, in my opinion, to be made with equal notoriety, if my feebleness would permit; and this consideration will, I hope, induce your Lordship to pardon the unusual liberty which I take in addressing you thus publicly.

But it seems to me proper, before entering upon this narration, that I should speak at greater length of the case of Dr. Stanger. Your Lordship's attention must have been so much occupied by the many important affairs, in which you have been engaged since its occurrence, that the traces left in your memory by some of its circumstances, the knowledge of which is necessary to the right understanding of what I have to say respecting myself, are now perhaps nearly, if not altogether effaced.

On the 26th of January, 1796, Dr. Christopher Stanger, a physician of eminence in this metropolis, made oath in the Court of King's Bench, that he had shortly before applied to the College of Physicians, to be examined for admission into their order of candidates; and that this examination had been refused to him in consequence of a by-law, which he conceived contrary to the intention of the charter and acts of Parliament, by which their corporation had been established. The next day, a rule was granted by the court for the college to show cause, why a mandamus should not issue to compel them to examine that gentleman. In the beginning of the following April, Sir George Baker, president of the college, and Mr. Roberts, their attorney, made each of them an affidavit, to justify the refusal to admit Dr. Stanger to the examination he required. In these affidavits it was stated, that the college, in pursuance of a power granted by their charter, had from time to time prescribed certain qualifications and conditions, as requisite for the admission of persons into the commonalty or fellowship, and into the order of candidates; that, by one of their statutes then in force, no person could be admitted into that order, unless he were a doctor in medicine of Oxford or Cambridge; that Dr. Stanger was not a graduate of either of those universities; and that there were two by-laws of the college, by which licentiates of certain descriptions might be received into the fellowship, without their previously entering into the order of candidates. Such were the general grounds on which the refusal of the college to examine Dr. Stanger was to be defended. It was, however, clearly seen from Sir George Baker's affidavit, that if the reasons hitherto alleged should be found insufficient, an attempt would be made to show, that the applicant was unworthy of reception into any society, from having violated the faith which, it was said, he had solemnly pledged to the college, upon being admitted a licentiate.

The by-law, which restricted admission into the order of candidates to the graduates of Oxford and Cambridge, had been decided by Lord Mansfield to be bad; and according to the confession of the counsel of the college, the two by-laws, which allowed licentiates to enter the fellowship, had been framed in consequence of the censure passed by that judge upon the former system of admission, and of his recommendation that a more liberal one should be adopted. Their real defence, therefore, as having regard to the possible applications of persons in whom they could not pretend to find the smallest appearance of blame, rested entirely upon the two last-mentioned by-laws.

These by-laws were recited at length in the affidavit of Mr. Roberts. By one of them, the president was allowed once in two years, but not oftener, to propose a licentiate of ten years standing, to be admitted into the college *without* examination of his fitness. If he chose, however, to omit the exercise of this privilege, as the present president has repeatedly done, it was not to devolve upon any other person. But when Lord Mansfield condemned the whole of the former system of admission, there existed a much more liberal statute for the reception of licentiates, through *favour*; for according to it *every* licentiate of *three* years standing, who had taken the degree of doctor in medicine, after studying four years in *any* university, might in this way be admitted a member of the college: one at least, therefore, of the new by-laws, certainly afforded no corrective to the evil, of which that great man complained.

The remaining by-law was consequently the only source, from which such a corrective could be expected. It declared, that licentiates of seven years standing, and who had completed the thirty-sixth year of their age, might be admitted into the fellowship of the college, should they be found fit *upon examination.* I shall not trouble your Lordship, at this time, with any observations upon the numerous fetters, by which the action of the pretended principle of this by-law was impeded. I have at present nothing in view but to show, that this was the only measure of any importance the college had adopted for the purpose of removing the reproach, which had been thrown upon them by Lord Mansfield; and that it therefore afforded the only good ground for their resisting the issue of the mandamus which Dr. Stanger solicited.

Accordingly, when the question of the mandamus came to be argued before the Court of King's Bench, on the 23d of April, 1796, Mr. Erskine, the leading counsel of the college, was found to derive from this by-law his chief reasons against the proceeding of that writ. It can scarcely be thought, that so ingenious and eloquent an advocate would confine the defence of his clients to any single point. It was not surprising, therefore, that he should make a show of resistance at various parts. But still it was evident, that this by-law was regarded by him as his only secure position. How could he indeed act otherwise, consistently with the deference which was due to the opinion of Lord Mansfield? Dr. Stanger had applied for a mandamus to the Court of King's Bench, because the College of Physicians refused to examine him for admission into their order of candidates. But the by-law, in which they grounded this refusal, had been decided to be bad by the late chief judge of that court. No other defence then could well be offered there for such conduct, than that, in compliance with the advice with which his censure was accompanied, a new by-law for the admission of members had been framed, which so qualified the former, as to take away from it all appearance of illiberality; and that if Dr. Stanger chose to apply under the new statute, he would readily be received into the college.

This appears to me a just summary of the chief, if not the only argument, of Mr. Erskine upon that occasion. But to avoid all suspicion of error, I shall now take the liberty of showing in what manner Mr. Erskine represented his own argument, and what assistance he expected to gain from it, in his attempt to prevent the issue of the mandamus. I shall at least prove by this procedure, that I can have no intention to deceive.

EXTRACTS from Mr. Erskine's Speech in the Court of King's Bench, April* 23, 1796, *in the Case of Dr. Stanger against the College of Physicians.*

" Subsequently to the time when Dr. Fothergill's case was before the court, there was a revision of the statutes of this learned body, who took the very best and the most eminent advice which this kingdom could furnish them."

" They made two by-laws—in which there is a power given for any fellow at the ordinary comitia majora, after Michaelmas, to propose a licentiate of seven years standing, who is thirty-six years of age, for examination, who, if approved of by the majority of the fellows then present, is to be examined at the three next comitias, and then, if approved, to be admitted

* From Mr. Gurney's Report, taken in short-hand.

a candidate, though he has not studied at either of the English universities.

"Your Lordship will observe, that Dr. Stanger could not have this mandamus under this by-law, and therefore I admit I must support the by-law Sir George Baker sets forth in his affidavit, because no person, except he be of one university or the other, can possibly be examined, but upon the proposition of one of the fellows that he should be examined; and upon the proposition of one of the fellows, if he be a licentiate of seven years standing, and thirty-six years of age, *though he has not that qualification which is required in the by-law set forth in Sir George Baker's affidavit, yet this door is open to him.* And can it be supposed, or will any gentleman stand up and say it is consistent with probability, that a man of eminent learning and high qualifications, who, notwithstanding he has not had that species of education, which I will show from the time of the charter to this day has been constantly adopted; yet, if he be a person who has undoubtedly qualifications for it, is it to be supposed that there is not one fellow of the whole college who would propose such a person?"

"In the by-law which I have just stated, any one fellow may propose the examination of an individual, though such individual could not, according to the ordinary by-laws of the college, be admitted to examination. But to leave the door open, and to prevent the observations that were made in the case of Dr. Fothergill, and under the auspices of the most learned men of the profession, this by-law was made; therefore, I think, I have gone the length of showing, that the present by-laws can be attended with no possible inconvenience."

"I will only ask my friends, by-and-by, to explain to your Lordship, how it is consistent with reason or common sense, to say, that the public can suffer, or this learned profession be affected in its dignity or advantages, if no person should have an opportunity to force himself into their college, unless he comes within the scope of their by-laws, sanctioned from all antiquity, and comes within the sense of these by-laws; although no door is shut against them at all, but any one fellow of the college may, notwithstanding that statute, propose them for examination, &c. It is not easy to conceive, that a man can be entitled to so much favour, because of his eminent qualifications, as that he can supersede all the rules and provisions of the country, and yet shall not be able to find one person within the walls of a college, consisting of near a hundred members, to propose him, although such a man would add dignity and lustre to the college."

"Will any man say that these things are attended with any inconvenience to the public? They are not at all; for, in the first place, if the gentleman who proposes himself to examination has studied at either of the English universities, then this does not apply; if he has not studied at either of the universities, and can find one fellow in the college who knows any thing of him, and thinks him a fit person to be proposed, then this by-law does not stand in his way."

Such, my Lord, was the use which Mr. Erskine made of this by-law, in resisting the issuing of the mandamus. The pleadings ceased almost immediately after he had finished his speech, and the rule was discharged, in consequence of an error which was discovered in the mode of Dr. Stanger's application to the college. While it was in doubt, however, whether this error was of sufficient importance to put a stop to the proceedings, some conversation took place between the judges and Dr.

Stanger's counsel, an exact relation of a part of which will demonstrate more strongly than I can possibly do, that the court uniformly regarded the conditions, which were required by the by-law for admitting licentiates into the college, merely as cautionary measures against the entrance of improper persons into their body; and constantly supposed, that if any licentiate of good character, and possessing the qualifications marked by the statute, could prevail upon a fellow to propose him, no obstacle would afterwards exist to his admission. How far these opinions were well founded, will hereafter appear to your Lordship.

Extract from Mr. Gurney's Report.

Mr. *Justice Lawrence.* "Where is the difficulty of a gentleman's getting some one fellow of the college to propose him?"

Mr. *Law.* "There has been no person admitted—there have been many trials, but nobody has ever got through that wicket, nor ever will."

Mr. *Justice Lawrence.* "Do you imagine, if they think Dr. Stanger, or any other physician, is a fit person, that they will not propose him?"

Lord *Kenyon.* "There is a wicket of that kind put in our own profession—for, as I understand, all the four inns of court have for some time insisted, that one of their body shall propose a gentleman to be called to the bar, and that precaution has been attended with extremely good consequences. I am sorry, from what one hears, that it has not been quite a sufficient guard now and then, through a little inadvertence or misinformation; but certainly it is attended with good consequences."

Mr. *Law.* "That is a delegation of the power of inquiry to one whose special business it is, and I believe every gentleman upon whom that delegation falls, discharges his duty properly, and makes that inquiry."

Mr. *Justice Grose.* "But why should not this supposed duty be as honourably and as well executed by them, as in our profession*?"

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* Students of law, as the author is informed, are admitted to the bar by the benchers of the inns of courts, who, for the following reasons, may be supposed to execute their trust with fairness and impartiality. 1. As they are few in number, each of them must feel himself responsible for the acts of the whole. 2. They are either of advanced age, and little connected with the practice of their profession, or of considerable rank in it. None of them, therefore, can well be jealous of any person who may apply for admission. 3. The applicants for admission are, for the most part, very young men, who for this reason cannot have exhibited such talents as are

The mode of Dr. Stanger's first application to the college having been determined to be

likely to excite jealousy in persons much their seniors, were these even liable to be affected with that passion. 4. The profession of law includes so many individuals, that the accession of one more to it can scarcely excite fear in any former member, that his profits may hence be diminished. 5. Since none are allowed to practise as advocates before admission at an inn of court, an applicant cannot, in the previous exercise of his profession, have given umbrage to any of those who are to decide upon his fitness. 6. So many gentlemen of great figure and independent fortune embrace the profession of law, either with the view of preparing themselves for the discharge of various duties incident to their rank in society, or in the expectation of obtaining some high office in the state, that it is natural to infer that great liberality exists in its government. Stronger reasons may no doubt be given by persons better acquainted with the subject, but these seem to the author sufficient to explain the fact, that every person, who possesses the prescribed qualifications, is morally certain, upon application to any of the inns of court, of being admitted to the bar.

Similar reasons cannot be given, why fairness and impartiality should be found in the decisions of the College of Physicians, upon the applications of licentiates for admission into their body. For, 1. Not a few of the fellows, but all of them indiscriminately, determine the fate of every such application. 2. The greater part of the voters are consequently not of such a rank in their profession as to be above the reach of jealousy. The proportion of such persons at the meetings of the college is further increased by their having little to do elsewhere. 3. As the seven years of the applicant's licentiateship will, in all probability, have been spent in the metropolis, it is surely not unlikely, that some of the voters wrong, in the June following he presented himself a second time to them, requesting permission to undergo any examination which might ascertain his fitness to be a *fellow* of their body. An examination was again refused. Dr. Stanger having made oath of this, a new rule was obtained on the 26th of November, from the Court of King's Bench, for the college to show cause why a mandamus should not issue against them.

may have become jealous of his talents or success. 4. The members of the college are very few in number when compared with the barristers belonging to all the different inns of court. In the list for 1798, there are only twenty-seven fellows who exercise their profession in London, and some of these are very aged, and take little concern in practice. Any advantage, therefore, to be derived by a licentiate from being admitted into the college, will probably be regarded by some of the former members as tending to diminish, or prevent the increase of their own emoluments. 5. In the course of seven years passed in the exercise of a profession, in which, above all others, misunderstandings are apt to arise among its different members, it is almost impossible that a licentiate should not have given umbrage to some of those who are to decide upon this application. 6. Physicians in this country are almost universally taken from the middle ranks of men. They cannot therefore be expected to conduct themselves, as a body, in the same liberal manner as the members of a profession, which contains a considerable number of persons of high birth and large hereditary fortunes. Other circumstances, tending in like manner to produce unfair and partial decisions in the College of Physicians, when licentiates apply to them to be examined, will be mentioned hereafter.

On the 23d of January, 1797, Dr. Gisborne, then president of the college, made an affidavit in answer to Dr. Stanger's, the purport of which was the same as that of the affidavits of Sir George Baker and Mr. Roberts, in the former cause. In the new trial which followed on the 11th of May, 1797, the leading counsel of the college, as in the preceding one, was Mr. Erskine, who quickly abandoning all weak points, again fixed upon the by-law for the admission of licentiates, after an examination of their fitness, as the only ground which was fit to bear his works of defence. To prove that I am here also justifiable in attributing such conduct to him, I proceed to insert several passages from his speech upon this second occasion.

EXTRACTS from Mr. Erskine's Speech in the Court of King's Bench, May 11, 1797, *in the Case of Dr. Stanger against the College of Physicians.*

"Your Lordship will take it that this last statute which I have read, [that restricting admission into the order of candidates to graduates of Oxford and Cambridge] and which still is in existence, and which is qualified by others I am about to state, was the last in existence at the time when the cases of the King v.

Dr. Askew and Dr. Fothergill, and those other cases came before the Court of King's Bench, as reported in Sir James Burrow. Since that time your Lordship will find that other by-laws have been introduced, greatly qualifying those previous by-laws, and as I have been given to understand, framed under the advice, and with the assistance of some of the most eminent and learned persons in this kingdom, in the profession of the law." [Mr. Erskine then recited the by-law, by which licentiates might enter the college upon being examined in regard to their fitness, and afterwards proceeded thus:] "Your Lordship observes then, that by the last by-law which I have just now stated, though a man had never seen either of the universities, yet if he can find out of the whole college of physicians, any one person who is a fellow of the college, to usher him in for an examination, he is, notwithstanding the other statute, of which this statute, your Lordship observes, is a great qualification, entitled to undergo the ceremonies which the college has thought fit to prescribe; and which I will show your Lordship, by and by, it has, and it cannot be denied that it has, a right to prescribe for its own government; he may be admitted."

"Then what are we assembled here upon? Why upon this grave and notable question—whether the by-laws which I have read to your Lordship, taken altogether as one body; *those that are subsequent qualifying, restraining, and modifying those that are antecedent; whether all these taken together constitute a reasonable body of by-laws, within the meaning of the charter*, granted by the king, and confirmed by an act of the legislature. Or, whether these by-laws shut out any persons who had a right by some privilege inherent in them as British subjects, under this charter, and this act of parliament, from becoming members of this grave and learned body."

"Would a mandamus go to the Bishop of London if he refused ordination to a person who set forth his learning, but had not been at either of the universities, and was therefore rejected? But have the college of physicians done that? No; they have done no more than this—if you have been at the university, and have acquired a degree and testimonials, without dispensation, we presume that you are learned from the place from whence you came, and the discipline you have been engaged in, and we examine you at once; but if you have not, *do* *we reject you? No; but we require that you should be introduced for examination by some one of the fellows of the college, and then we will examine you.* Is it consistent with common sense to say, that there is any thing unreasonable in that?"

"Your Lordships have the same authority, assembled in your judicial capacity, as judges over our voluntary societies, as you have over a college by mandamus. I apprehend, if a person were to apply to your Lordships, and say, I have been rejected at Lincoln's-Inn; why? because I could find nobody who would give in my name to the benchers to be called to the bar; you would reject such petition with indignation. You would say, that those learned bodies, who have a jurisdiction exactly similar, only that it is directed and referred to a different profession, in the regulation, and in the learning and integrity of the members of which, the public have a similar interest, inasmuch as they exercise a profession very important in every view of it; your Lordships would say, that he ought not to be admitted, who could not find one person to propose him as fit to be examined; (*and that is all that we are contending for*) because if a man can find any one fellow of the college to propose him, he may be admitted, provided they think him fit."

"Now I will consent to the learned Serjeant making this rule absolute, if he can prove that this by-law is unreasonable; for we are here upon the reasonableness of the by-law. I read that part of the charter which gives them authority to make by-laws, and I defy the wit or imagination of man to put another question upon the court here, than—Whether this class of by-laws, *taken altogether*, be unreasonable. I consent to the rule being made absolute, if any one of my friends, or all of them together, can, in their imagination; I do not appeal to any experience they can bring; but if they can in their imaginations, however fertile they may be, figure to themselves an inconvenience that may arise from them. They may say, Oh, there may be a conspiracy which may exclude a virtuous and enlightened man! Setting aside the main improbability, that *members of a learned body could league themselves in a conspiracy so base and so scandalous, as to refuse to examine a man proposed to them by one of their own order, under their own laws*, from a professional jealousy, lest they should be eclipsed by that person, &c."

"And yet what is the argument, that, when bowing to the great learning and ability of Lord Mansfield upon that occasion, when the college having no other end and object in the world; and what other end and object can they have, than the regulation of a profession, which I will say—and let Dr. Stanger take part of the honour if he pleases—is a profession which not only preserves the health of our relations and friends, and gives greater security to human life, but which, I say, also gives us a class of men who are an ornament to society and to this country, with a knowledge of the languages and the various branches of philosophy, which gives that insight into nature and its works which are acquired in the learned institutions, which now are to be broken down, and all sort of persons are to be suffered to do—What? Not to practise physic, for they practise it already, but they are to be let in for the purpose of governing one of the wisest and the most learned bodies; of governing men who, one and all of them, almost, are deeply skilled in every thing that learning and science have brought forth in any age; and yet, forsooth, it is to be considered, as if the charter and acts of parliament were likely to suffer, *because a man has kept his learning so much to himself, that nobody could ever find it out, so as to be able to think it was wise or decent* *to propose him*; or else, that he is such a phenomenon in human shape, that there must be a conspiracy among them to keep him out, lest he should eclipse them all. I am sure Dr. Stanger does not wish to represent himself as such a person; *but I am certain that if Dr. Stanger would have applied to the college*, as men of the first learning in every age have applied to it, *he would have been admitted*."

These were the arguments, which the by-law for admitting licentiates to examination furnished to Mr. Erskine, against the issuing of the mandamus, upon Dr. Stanger's second application. That they were the only arguments, upon which he placed the least dependance in preventing that writ from going forth, is demonstrated by the concluding sentence of his speech, in which he collects to a single point the scattered tendencies of all that he had before advanced. "My proposition is," said Mr. Erskine, "that it is reasonable the college should say; if you are of the universities we will examine you at once; *if not of the universities, we do not refuse to examine you*, but we consider it reasonable to point out the mode in which that examination should go forward; otherwise we must examine all the world: and we conceive that the regulation which we have imposed, in order to prevent frivolous examinations, is not inconsistent with the reasonable exercise of discretion; and which, therefore, is warranted by the charter, which entitles us to make these statutes."

Having already occupied so much of your Lordship's time in citing passages from Mr. Erskine's speeches in the case of Dr. Stanger, I feel averse to give extracts from those of the remaining counsel of the college upon the same occasion. I shall, therefore, only recal to your Lordship's recollection, that two of them, Mr. Gibbs and Mr. Dampier, made use of the same by-law to convince the court, that the mandamus ought not to proceed.

I know not, my Lord, exactly in what light the declarations of counsel in a court of law are to be regarded, or how far they may be thought binding upon the persons in whose behalf they are made; but if they are ever held to impose an obligation upon a client, and without admitting that they do, it is difficult to conceive how the business of a court of judicature can be carried on, those in Dr. Stanger's case ought to have produced this effect. The principal advocate, of a body termed by your Lordship, *the sanctuary of honour and good faith*, declares to the Court of King's Bench, that his clients are willing to examine Dr. Stanger, or any other person of the same description, should application be made to them under a particular bylaw which he recites. This pledge, for so I must call Mr. Erskine's declaration, was given in April, 1796. The same cause was tried a second time in May, 1797. If, therefore, Mr. Erskine had gone beyond his instructions in giving that pledge, sufficient time had surely intervened, to have allowed the college to make the discovery, and to warn him against committing the same error a second time. Did this happen? His subsequent conduct proves that it did not; for in his second speech he repeats the pledge, in language still stronger than that which was formerly employed by him. But it may be said, that inveterate obstinacy, or unconquerable ignorance on the part of Mr. Erskine, might occasion the repetition. Some of a committee of the college appointed to conduct the law-suit hear Mr. Erskine's second speech. Do they then caution the remaining counsel to avoid the rock, which had twice endangered the safety of the vessel committed to his care? We can here also only judge from the event. The two who speak next, vouch, like Mr. Erskine, for the willingness of the college to examine Dr. Stanger, or any other person of similar qualifications, for admission into their body.

But it seems superfluous to offer proof, that the college were bound by the repeated and unchecked declarations of their counsel, to a prompt and honourable execution of the statute for the admission of licentiates to examination, when it is considered in what light that statute was regarded by the court. For in delivering your opinion on Dr. Stanger's case, your Lordship, after speaking of the by-law which had formerly restricted admission into the college to the graduates of Oxford and Cambridge, was pleased to express yourself in the following manner:

"If it, [the restrictive statute] had been a sine quâ non, if it had controlled the parties who are to form their judgment, and taken from them all power of decision upon candidates, it would have had that seed of death in it, which *Lord Mansfield found in that by-law* which he decided to be bad.* But this is not so; here

* The only difference between the present restrictive statute, and that which was in existence in the time of Lord Mansfield, is, that foreigners, who have taken degrees at Oxford or Cambridge, are not now prevented from entering the college: but it is evident that this relaxation can affect very few persons, perhaps not more than one in a century.

every person has *a right to address himself to the honourable feelings of those breasts*, to which Dr. Stanger must at last have addressed himself, if this mandamus went. If they find him to be, (as I am inclined to believe he is from what I hear of him) possessed of all the requisites of medical learning and moral character, he will address as powerful arguments to those gentlemen, *every individual of whom is called upon to exercise his opinion upon the subject.* He is not to wait to be seconded; the by-law does not require that; if any one proposes him the question is submitted to a majority. It goes then to that tribunal, which, I hope and believe, is the *sanctuary of honour and good faith, and he may as well address himself to them now as if this mandamus went*; they are not bound to admit, all they are *bound to do is to examine.*"

One of your brethren on the bench, Mr. Justice Grose, refused the mandamus on the same ground as your Lordship. Another, Mr. Justice Lawrence, had several times, in the course of the two trials, declared his confidence in the readiness of the college to admit any licentiate, in the situation of Dr. Stanger, to an examination, and for this reason probably thought it unnecessary to repeat the same opinion, when he gave his reasons for refusing the mandamus. The remaining judge, Sir William

Ashurst, was the only one who did not, at some period or other of Dr. Stanger's applications to the court, approve of the by-law for the admission of licentiates into the college upon examination, and express his belief that it would be carried into execution, whenever an application should be made in consequence of it. What he said, however, at the close of the trial, afforded no reason to suppose, that he entertained sentiments on these subjects, different from those of his brethren.

I have now, my Lord, finished the relation of those parts of Dr. Stanger's case, which seem to me to form a proper introduction to what I shall say concerning myself. I may have been tiresome by minuteness of detail; but if I have been at the same time accurate, as I believe I have, I trust that I shall readily receive your Lordship's forgiveness; more especially when it is considered with what view that statement has been given. It is to point out, in a manner not to be questioned, what conduct the college were bound to pursue upon the application of a licentiate for examination, before I describe the conduct which they actually did pursue, when such an application was made. It is to exhibit a picture from the masterly hand of your Lordship, guided rather by the suggestions of a warm and virtuous fancy, than by an accurate knowledge of the object to be represented, before I produce another picture of the same object copied from nature, by an artist, rude indeed and unskilful, but whose diligence and fidelity may have compensated his want of genius and taste.

Before the decision of the Court of King's Bench, in the case of Dr. Stanger, I had with many others believed, that the fellows of the college never meant to admit any licentiate to an examination. But when I had become acquainted with the declarations of their counsel, respecting the by-law for examining licentiates, and with your Lordship's opinion, that it furnished a remedy for the evil in the former system of admission, equal to that which even a mandamus could afford, I concluded with some firmness, that although my belief had been originally well founded, still they would scarcely be hardy enough to refuse to examine a licentiate, while the circumstances of Dr. Stanger's cause were recent in the memory of every one. Not having been in court myself during the trial of that cause, my first knowledge of the proceedings in it was derived from verbal reports. Fearing, however, that these might be incorrect, I thought it prudent not to form any plan in consequence of what had passed there, before I should see an account of the proceedings, which Mr. Gurney was then preparing from his notes in short-hand. When I had perused that account which from various circumstances I did not receive until several months after the trial, I hastened to inquire, whether any licentiate, who came within the conditions of the by-law, meant to avail himself of it: but, finding that there was none, I determined to apply for examination of my own fitness to be a fellow of the college, whatever reason I might have for being fearful of its issue, rather than allow the grounds of the decision to run any hazard of being forgotten, from want of an early appeal to them. I mentioned this intention to two of my friends among the fellows, Dr. David Pitcairn, and Dr. Matthew Baillie, who, by immediately offering to propose me, removed the first, and in the opinion of the judges of the Court of King's Bench, the only difficulty in the way of obtaining my object. On the 29th of September, 1797, a motion was accordingly made at the college by Dr. Pitcairn, and seconded by Dr. Baillie, not that I should be admitted a fellow, but merely that I should be examined concerning my fitness to become one hereafter. If your Lordship's surprise would have been excited, as surely it must, by any opposition whatever to this proposal, to what height will it be carried, when you learn the ground of that which was actually made? Could your Lordship have even imagined, that a by-law of the college of Physicians, which, by the declaration of their counsel in the Court of King's Bench, had been framed in 1778, with the best legal advice this country could afford, for the express purpose of removing the blame which had been thrown upon them by Lord Mansfield; that a by-law, which, if before forgotten, had been recalled to their recollection in 1789, by an application under it from Dr. Sims; that a by-law, to whose existence they had twice sworn before your Lordship, once in April 1796, and again in January 1797; that a by-law, upon which they had rested their chief defence in a recent trial before the Court of King's Bench; that a by-law, to the beneficial operation of which Dr. Stanger had, in the course of that trial, been advised by one of the judges upon the Bench to trust implicitly, instead of applying to the court for a mandamus; and, lastly, that a by-law, which your Lordship had expressly said *bound* them to examine every person who applied under it, should in September 1797, be declared a *dormant* by-law, the propriety of whose revival formed a question of very great concern, and was consequently not to be decided upon before it had undergone much serious consideration? In the midst of your indignation against such conduct in a body of men, formerly styled by your Lordship, *the sanctuary of honour and good faith*, it must yet afford you some consolation to know, that many of the members were free from its guilt; and that when a motion was made to get rid of Dr. Pitcairn's proposal, by what is termed the previous question, out of twenty-three, the whole number at the meeting, ten voted against it.

Few men are so lost to shame, as not to desire that their most iniquitous acts should wear an appearance of justice. It is not, therefore, wonderful, that the college of Physicians should have attempted to palliate the conduct which has been mentioned. With this view they maintained, that proper notice had not been given of the intended proposal by Dr. Pitcairn. But no such notice was either required by the by-law which authorised that proposal, or had been established by custom. What end indeed would the giving of notice in the case before them have served? Not surely to afford time for their considering, whether they were to receive what they were bound to receive. If it were to have relation to the character of the person to be proposed, all that could be well known of it was already in their possession; since, during the nine years of his being a licentiate, he had never absented himself from London an entire day, and had in the same time associated more with fellows of the college than with licentiates. Besides, the mere admission to an examination did not prevent the free exercise of their judgment at any one of the four other ballots which were to take place, before he could be received into their body; and as the last of those ballots was not to be held till twelve months after the admission to be examined, those of the tenderest consciences were allowed sufficient time for the most scrupulous inquiries respecting him.

But not to dwell longer upon this mode of answer to their pretext, I proceed to assert, that notice *was* given to the college of Dr. Pitcairn's intended proposal. If the proper person for receiving it was absent from his duty, the fault lay with him. Among the many illiberal circumstances of the by-law for admitting licentiates to an examination, is this; that no person can be proposed under it, except upon one day in the year; namely, at the general meeting of the college, immediately after Michaelmas. I had not been able before the 20th of September, to ascertain whether or not I could be proposed in 1797. Two days after this, and seven days before the meeting of the college, I went to the president's house in London, to inform him of what was intended, being desirous that, although such a notification was not required, it should not afterwards be said, that an attempt had been made to surprise the judgment of the college. I was told there that he was in the country, at a considerable distance from London, but that he was expected to return in a day or two. Upon this, I wrote a letter at his house, which I left there, to acquaint him with the object of my visit. Three days after, however, I learned that he was still out of town, and probably would not come to it till the day preceding the meeting of the college. In consequence of this information, I immediately sent a letter to him in the country, to make known what was meant to be done. On the same day I called upon the officer of the college, whose business it is to summon the fellows to their meetings, and authorised him to acquaint those whom he should see, that I was to be proposed for examination. I gave the same information myself to one fellow, my colleague, Dr. George Fordyce. If I did not give it to more, this was from fear, lest the doing so might be regarded as an indirect solicitation of votes. Yet, notwithstanding all this supererogatory care to apprize the president and fellows of the college of what was intended by Dr. Pitcairn, they were bold enough to refuse even to allow his proposal to proceed to a ballot, on this pretence, among others, that it had not been properly notified to them.

Amongst the voters against a ballot on the proposal of Dr. Pitcairn, was Dr. John Burges*, whose conduct in this matter seems worthy of particular notice, as he had himself only a few years before made a similar motion regarding another licentiate. I dispute not here the claim, which that gentleman makes to ancient faith, and purity of manners, and most exemplary zeal for *the honour* of the college: but as an humble inquirer into the principles of human

* I here, and perhaps shall elsewhere, venture to say, upon which side of a question a particular member of the college has voted, though it be the custom of that body to collect suffrages by ballot. But ballots are so little adapted to the freedom of Englishmen, that they are seldom in this country attended with the concealment, which is probably derived from them among the crafty and dissembling Italians. When votes are collected here, in this way, many of those who give them openly mention the side which they support; others, though they do not make a direct confession, yet by the tenour of their conversation, leave little doubt upon the same point; and by these means, as little doubt at length remains in regard to the few, who have endeavoured to involve their conduct in mystery. I shall be very ready, however, to correct any mistake which I may fall into upon this subject.

nature, I think myself entitled to say a few words upon his acting so differently at different times, in circumstances apparently the same.

Travellers inform us, that many of the inhabitants of the isthmus, which connects the two great continents of America, labour under a most strange depravation of sight. When the sun has arisen above the horizon, and has enabled other men by its light to pursue their ordinary occupations, these people become blind, and retire into caverns and dark woods, there to pass the day in quiet and repose. But, as soon as night has descended upon the earth, and the face of nature is to other eyes covered with darkness, their sight is restored, and they then come forth from their hiding-places, to exercise the labours, and enjoy the pleasures of life. I know not whether a similar infirmity has hitherto been observed in the mental perceptions of man; if it has not, I announce the existence of an undescribed disease, and produce the conduct of Dr. Burges in proof of my discovery. This gentleman, some years ago, saw so clearly the propriety of carrying into execution the by-law of the college, for admitting licentiates to examination, that he proposed Dr. James Sims, as a candidate under it. To the other fellows, however, the propriety of the measure was then involved in so great darkness, that no one of them could be induced to second his motion. In 1797, the arguments of the advocates of the college, and the speech of your lordship, diffused such light over this subject, that when another licentiate is proposed for examination, ten fellows, without the smallest solicitation from any person, and in direct opposition to the suggestions of ancient prejudice, declare their opinion, that it ought to be granted. But the light which now enables men of healthy minds to discern merit in a measure, in which they formerly could see none, overpowers by its excess the infirm perception of Dr. Burges. In this distress, groping in darkness, he begs for time to consider, whether the by-law for the examination of licentiates, ought even in *any* instance to be carried into effect.

At the time that I was made acquainted with the fate of Dr. Pitcairn's proposal, I was also told, that since it had not been put to a vote, it could not be said to have been *rejected*, but was rather to be supposed still lying on the table of the college. Hence I concluded, that if a charge were now brought against them of disregard to the decision of your Lordship, they might attempt to evade it by maintaining, that the consideration of Dr. Pitcairn's motion had been only *suspended.* I resolved, therefore, to bring their sincerity to trial afresh, by having myself proposed a second time for examination. Upon mentioning this determination to Dr. Pitcairn, he offered his aid in accomplishing it, by repeating his former motion in September 1798; before which, in consequence of what has already been observed, it could not be received.

During that interval, the college proceeded to impose a new restriction upon the admission of licentiates into their body, as if to demonstrate the truth of the allegation against them, which had been so scornfully repelled by the judges of the Court of King's Bench, that their by-law for the examination of persons of that class was altogether illusory, and had been framed with the intention, that no one should ever be received by it.—The new restriction was, that whoever meant to propose a licentiate for examination, should give notice of this at a preceding quarterly meeting of the college. Its professed object was to allow time to the fellows for inquiry into the character of the person to be proposed. The pledge to be given by a fellow upon proposing a licentiate, the candidate's residence for at least seven years in the midst of them, and the interval of a twelvemonth between the first and last ballots upon his fitness, were consequently declared to be insufficient barriers against the entrance of unworthy persons into the corporation. But it seems to me more difficult to admit that this was in truth their opinion, than to believe, that the real object of the new regulation was very remote from the one exhibited; and what I shall immediately say, will probably incline your Lordship to form a similar conclusion.

In the end of June 1798, Dr. Pitcairn, though much debilitated by a dangerous illness, under which he had lately laboured, attended at the college to give notice, that he should in the following September again propose me for examination. To this notice he premised, that he conceived it to be unnecessary, since the merits of his first proposal had not yet been considered. But unfortunately for mankind and himself, he was shortly after again taken ill, and was in consequence obliged to leave London for the recovery of his health, a few days before the time arrived for making his motion. Previously to his departure, however, he wrote a letter to Dr. Baillie, in which, after stating his own inability to propose me, he delegated that office to him. Accordingly, Dr. Baillie produced this letter at the meeting of the college in September, and then proceeded to execute his trust. This was resisted by the same men, who had opposed the former motion for my being examined. It was urged by them, that the new by-law required the proposal to be made by the very person who had given notice of it. To this it was answered, that as the avowed object of the notice was to allow time for inquiry into the character of the person to be proposed, the spirit of the by-law prescribing it had, in the present case, been completely satisfied. And it was asked, whether a delegation had never formerly been received, when he who had declared his intention of bringing forward any measure was prevented by illness, or the unavoidable duties of his profession, from attending at the college to propose it. No reply was made; but a question was immediately put, whether the *present* delegation should be admitted. A ballot being taken, twelve votes were found against the delegation, and nine in favour of it.

An attempt was then made to bring in a different way before the college the original question of examination. It was maintained, that the first proposal by Dr. Pitcairn was still upon their table, as it had never been decided upon, and that it ought now to receive their determination. The minutes of the meeting in September 1797, were in consequence called for and read, upon which it was declared, that Dr. Pitcairn's proposal had then been *finally* *disposed of and rejected.* No cloud, therefore, now hangs over the conduct of the college; nothing now intervenes to alter its natural colours, or to distort the light by which it is seen.

Your Lordship, perhaps, notwithstanding the facts which have been described in the foregoing narrative, will scarcely think it possible, that the college of Physicians should have intentionally violated their engagement, or have advisedly acted in contempt of the grounds of a decision in the Court of King's Bench; and hence you may imagine, that they were influenced by some well-founded objection to the person proposed for examination, though from ignorance or inadvertence, they might have given to their conduct the appearance of a desertion of principles, which they were bound to maintain. Lest, therefore, you should be induced by your ancient respect for the college, to form an opinion so unfavourable to my cause, I will now attempt to prove, that no well-founded objection did exist to my being examined by them; I mean, no objection, which any of those who resisted the proposal for an examination, would venture publicly to avow. In such an undertaking I must necessarily speak much of myself; but for this I hope I shall readily be pardoned, since I stand now before your Lordship in the situation of one accused, and hence acquire a right of producing whatever testimony I can collect in my favour.

By the charter of the college, the qualifications required for its members, are learning and good character. In addition to these, a by-law demands from licentiates that they be of seven years standing, and thirty-six years of age, before they can be proposed for admission by means of an examination. Since, therefore, I possessed, without dispute, the latter requisites, all the avowable objections, which, in ordinary times, could possibly be brought against me by the college, are reducible to two kinds; one containing those which have any relation to my learning; the other, such as are connected with my moral reputation.

When I was proposed at the college by Dr. Pitcairn, all that was asked was, that they would examine whether I possessed the proper degree of knowledge for a fellow. Nothing more, therefore, on this head, could reasonably be required by them, before the trial, than a strong presumption of my being able to undergo it. Now this presumption was manifested to them in various ways. Their own advocates had asserted, in the case of Dr. Stanger, that the charter of incorporation, though it divided the physicians of London into two classes, members of the corporation and licentiates, demanded however the same learning from both; and that the college would act contrary to their duty, if they gave equal liberty to practise medicine to descriptions of men possessing unequal degrees of ability*. But, nine years previously to my being proposed by Dr. Pitcairn, I had undergone the trials of fitness, to which licentiates are subjected before admission to practise, and if I may venture to credit what was said by Sir

* This is a dictate of common sense; but though found by the counsel of the college, in the charter which was granted to them nearly three hundred years ago, its justness was not acknowledged when the late Dr. Fothergill became a licentiate; for he was permitted to exercise his profession in London, under a by-law which declared, that one reason for constituting a class of licentiates was, that many persons who were fit to practise medicine, had not, however, sufficient learning to be fellows. But there is reason to believe, that the late admission, on the part of the college, of equality in point of learning between the fellows and licentiates, was merely to serve a particular purpose during the trial of Dr. Stanger's cause. For in the testimonials of fitness to practise, which they give to licentiates, they still refuse to style them doctors of physic, though they constantly bestow that title on fellows; and it was, I suppose, in consequence of this distinction, that a president of the college had the effrontery to tell a learned professor of Gottingen, when upon a visit to this country a few years ago, that the licentiates of the college were not *proper* physicians.

George Baker, and the censors who examined me, I had passed through those trials with more than ordinary ease. In the interval, I had become a member of the Royal Society, the certificate of my fitness for which was signed by the late and present presidents of the college, Sir George Baker, and Dr. Gisborne, and by four others of the present fellows of that body. During the same interval, I had endeavoured to extend the boundaries of our knowledge in various parts of natural philosophy; and two of my attempts of this kind, certainly not the most considerable, had been recorded in the printed transactions of the Royal Society. As I had thus demonstrated industry at least, in the cultivation of sciences collateral to medicine, it is not probable that I had been inattentive to the study of my own profession, since my peace of mind necessarily depended upon my understanding it. Nor had my opportunities of gaining experience in it been very small; for I had been eight years a physician to an extensive establishment for the relief of the sick poor, and I had also been physician for some time to another institution of the same kind, but still more considerable. From all these circumstances, I think it will readily be allowed by your Lordship, that it was not likely I had become less learned since passing the trials of a licentiate, and that consequently there was a strong presumption of my being sufficiently learned to be admitted to undergo the additional tests of knowledge, if there be any such, which the statutes of the college demand from those who desire to be fellows. This will be the more readily granted, when it is considered, that though the college contains at present many learned men, and will no doubt continue to contain many such, as long as the inhabitants of this country are sufficiently rich to reward liberally the professional labours of physicians, yet the degree of knowledge which is just sufficient to enable any person to enter their body, cannot be regarded, even by themselves, as very high: For,

First, among the forty-three members who have undergone the required examinations, however they may have differed in original talents, industry, opportunities of studying their profession and in modesty, there is only one, whose learning is said to have been declared insufficient upon his first application for admission:

And secondly, the three physicians, who to my poor apprehension have appeared to have the weakest understandings and the smallest extent of knowledge, of all those with whom I have happened to converse, either in this or any other country, are fellows of the college of London.

I come now to the objections which might be brought against me on account of my moral reputation.

How far my previous life had entitled me to a reputation for good morals, it does not become me to say; and I am for many reasons unwilling to exhibit the direct testimony of my friends upon this part of my character. Such a step, indeed, seems on the present occasion altogether unnecessary, as I think I can easily prove, that the majority of those who formed the meeting of the college, when Dr. Pitcairn proposed me for examination, did not conceive me unfit to be received into their corporation, by reason of my immorality.

It will, I suppose, be readily granted, that as many, at least, as voted for receiving Dr. Pitcairn's proposal, entertained no objection to me, on the ground which has just been mentioned. Now, the numbers on the different sides of the question, when his proposal was rejected, having been thirteen and ten, if I can only show, that *two* of the thirteen had shortly before manifested their satisfaction with my character for morals, the object at present in view must, in my opinion, be looked upon as gained.

Dr. Gisborne, the president of the college, who I venture to maintain voted for the rejection of Dr. Pitcairn's proposal, some years ago, as has already been mentioned, signed a certificate of my fitness for being a fellow of the Royal Society. Now, as a good moral reputation is always esteemed there a necessary ingredient of fitness, he must certainly have then believed me to be possessed of that qualification. That he professed a similar belief, only a few months before Dr. Pitcairn proposed me, I can assert upon the authority of Dr. James Robertson, a fellow of the college, at present with his Majesty's forces in Minorca; and I take upon myself to say, that nothing happened in that short interval, which ought to have lessened it.

Sir Lucas Pepys was another of the fellows who voted for the rejection of Dr. Pitcairn's proposition. In the beginning of the year 1797, I appeared before the Board of Censors of the college, to complain of irregular conduct in an apothecary, who was also present to answer to my charge. Sir Lucas Pepys, then sitting as president of a court, the members of which are *sworn* to do justice, addressed the delinquent in a grave and solemn speech, in the course of which he delivered these words: "Dr. Wells is no mean person; he is well known to the world both as a gentleman and a scholar." Whether this opinion be just or not, is at present no matter of inquiry. But in tenderness to Sir Lucas Pepys, acting as a judge, under the solemn obligation of an oath, it must be supposed that he really entertained it. In like manner as when I spoke of Dr. Gisborne, I venture here to affirm, that nothing occurred in my conduct from that time to the 29th of September in the same year, which should have induced Sir Lucas Pepys to alter his opinion of me as a gentleman.

I might proceed to show, my Lord, that other fellows of the college refused to receive Dr. Pitcairn's proposal, upon grounds that had no connexion with my moral reputation. But, as what I have already said appears sufficient for attaining the end I proposed, I quit with joy a subject so distasteful, and betake myself to one more congenial to your Lordship's feelings, the consideration of the support which was given to my fitness for being received at the college, by the characters of him who made, and of him who seconded the proposal for my being examined.

One of those gentlemen must already be well known to your Lordship. I cannot, however, refrain from saying respecting him, that the son of the gallant Major John Pitcairn, who died the glorious and enviable death of a soldier, fighting for his country, and the adopted son of the high-minded, upright, and generous Dr. William Pitcairn, must have every title to the strictest honour, which inheritance, education, and domestic example can bestow. But why do I speak of titles, after his countrymen had acknowledged his complete possession of that most invaluable property, and had in consequence, as well as from their high opinion of his learning and skill, placed him at the head of the profession of medicine, in the metropolis of Great Britain*?

He who seconded the proposal, Dr. Matthew Baillie, is more upon a level with myself, in regard both to age, and length of residence in London. Somewhat, therefore, of the obscurity which involves almost every young physician, may have hitherto concealed him from your Lordship's notice. But that obscurity is fast dissipating, and he must soon, my Lord, very soon, appear to your view, with all the just

* Two circumstances must concur to place a physician at the head of his profession in London; 1. Great employment, which alone, is certainly not sufficient for that purpose, as it is often possessed by persons of no considerable ability. 2. Respect from other physicians, indicated by their frequently requesting his aid in their practice. This can arise only from a high opinion of his honour and skill, of which qualities in a physician, scarcely any but those of his own profession have either opportunities or capacity to judge rightly. Dr. Pitcairn, from the death of Dr. Warren to his own unfortunate illness, was indisputably the physician in London, in whom those circumstances existed together in the greatest degree.

proportions and accurate lineaments of a man of integrity, learning, and great professional skill.

Can it be conceived then, my Lord, that such men were ignorant of the character of one, with whom they had been acquainted for many years; or, that believing it to be unfit for mixture with the college, they would yet pledge their own honour upon its pureness? None scarcely are so depraved as to do wrong for its own sake; temptation is for the most part necessary to induce the most abandoned villain to add to his crimes. Supposing now for a moment, that Dr. Pitcairn and Dr. Baillie were capable of being actuated by unworthy motives, they could not have possibly gained aught by proposing me. They could not desire to get rid of importunity, for what they did was of their own free motion; or to repay favours which had been received by them, for on the score of good offices I was already greatly in *their* debt. On the other hand, they knew well, that what they were doing was highly disagreeable to the governing members of the corporation. These men they were obliged to meet frequently, either in ordinary society, or in the exercise of their profession, or at the comitia of the college. It was, therefore, of importance to their ease and comfort at least not to offend them. Since, however, they did offend them, without deriving the smallest advantage to themselves from their conduct, they must necessarily have had the firmest conviction of its rectitude; and in this conviction I find the strongest proof I can offer, that in point both of learning and moral reputation, I was not unfit to be examined for admission into the College of Physicians of London.

I have now, my Lord, considered the two grounds, upon which the college, consistently with their charter, might possibly have regarded me as unfit for admission into their body. But perhaps it will be said, that they drew their objections to me from a source different from either of those which have been mentioned; that they believed me infected with the madness of the present times, and desirous of entering their corporation, for the purpose of assisting more effectually to destroy it, along with every other ancient establishment in this country. Such at least were the principles of conduct attributed by many of the fellows of the college to those licentiates, who had engaged in the scheme of opening the corporation to every physician of learning and honourable character. Even after Dr. Stanger's cause was determined, when apparently no object to be gained by calumny existed, one of the fellows, Dr. Robert Bourne of Oxford, a gentleman, as

I have since known, of great prudence, and of the mildest manners, and who was then probably not acquainted with any one of those licentiates, placed notwithstanding a revolutionary spirit among the reasons which were assigned by him, in a public oration, for their attempt to gain admittance into the college. Nothing can more strongly demonstrate the pains, which had been taken to propagate such slander, than its having been received, credited, and still further spread by Dr. Bourne.

Opinions, leading to the overthrow of the monarchical part of our constitution, have long existed in this country, in a greater or less degree; but since the termination of the grand rebellion, they have been, till very lately, almost entirely confined to a few speculative men, who have shown little desire to gain proselytes, or in any other way to attempt a completion of their fanciful projects. Neither therefore the college of Physicians, nor, I believe, any other of our corporations, ever formerly refused to admit a person among them, merely on account of his notions of government, provided he had complied with the forms which were prescribed by the laws of the country, or their own private regulations. But the modern holders of republican principles, if indeed the workers of confusion can be said to possess principles, and if what tends to the misery of the whole can be denominated republican, follow a far different course. They labour with an apostolic zeal to impress their tenets upon others. No fancy is so wild as to be refused admittance into their minds; and whatever exists there is regarded by them as a legitimate cause of action. To employ the influence which they derive from places of trust under an ancient government, as a means of subverting it, is with them a duty; their great ambition is to show, that they are ready to sacrifice friends, family, and country, to obtain their beloved object, the destruction of order. It appears, therefore, highly proper, that the guardians of the different public establishments, to whom any discretion is in this respect allowed by law, should resist the entrance of every person, who notoriously holds opinions unfriendly to their existence. But though this be granted, it surely ought not to follow, that a vague surmise, an unauthorised suspicion of disloyalty, should operate to the exclusion of any one from a situation of honour or profit, to which he is otherwise legally entitled. Envy and malice in their native forms have considerable influence over human affairs; if permitted to assume the shape of patriotism, their power must be irresistible.

Upon what ground the college charged the licentiates with being disaffected to the constitution of their country, I know not. It was clearly no proof of their being so, that they appealed to the Court of King's Bench, for a rigid execution of a charter, which had issued from the most tyrannic prince of the despotic house of Tudor; or that they founded their title, to what they prayed the court to grant, upon the interpretation given to that charter by your Lordship's immediate predecessor, Earl Mansfield, certainly no friend to levelling principles, or to seditious combinations of men. Perhaps the proof was derived from this circumstance, that no one of the licentiates who signed the address to the college, in which they set forth their right to be examined for admission into the corporation, either enjoyed, or expected to enjoy, any professional honour or advantage directly connected with the present government of the country. "Is it probable that these men," the fellows of the college might say, "who are attached by nothing special to the existing constitution, can desire its continuance? Our own bosoms declare that they cannot; they must, therefore, be labouring to subvert it." But the pampered Rich basely deserted his master in the hour of distress, while thousands of our countrymen, bound to their sovereign by no other tie than their allegiance as Englishmen, fought and died in his defence. From whom did the expiring cause of royalty in France receive its last support? Not from the pensioned courtiers of Versailles; but from a Stoflet, and a Charette, men before unknown, but whom the occasion that called for their talents formed into heroes; from the plain and simple inhabitants of Brittany, actuated by no motives but what arose from attachment to the ancient government of their country, and reverence for the religion of their fathers.

Leaving, however, to more able advocates, what further defence may be deemed proper for the other licentiates, who have been charged with disloyalty by the members of the college, I shall now confine myself to a special vindication of my own character from so atrocious a calumny. If, my Lord, I speak with warmth upon this subject, I trust that I shall find an excuse in the energy of your own feelings. He that is wealthy may be robbed, without knowing that he has experienced an injury. But the poor man's all is often included in a single object, which, though to other eyes worthless and contemptible, may be to him the sole spring of joy and hope. Any attack upon it excites his utmost powers of resistance; its loss leaves him without bond to the world, or interest in its concerns. When we read of a rich man's despoiling a poor neighbour of his only property, "one little ewe-lamb which lay in his bosom, and was unto him as a daughter," our sympathy with the sufferer is nearly as great, as if he had been a monarch unjustly expelled from his dominions. I may well then be allowed to feel acutely the attempt which has been made to strip me of almost my only possession, to which my title is founded upon paternal discipline and personal suffering, and has been illustrated by the whole tenor of my life.

I was born, my Lord, in Charlestown, in South Carolina, but my parents were from Scotland. My father, who was a man of observation and a scholar, though a tradesman, had carried with him those opinions respecting the kingly branch of the British constitution, which in the former state of our parties constituted Toryism; and the resistance they met with in a country, the inhabitants of which were, from their situation, always somewhat inclined to republicanism, served only to strengthen them. These opinions he early endeavoured to impress upon myself. To remove, however, every fear of my being infected, from my companions, with the factious and disloyal principles, which had very generally pervaded the British Colonies in America, from the conclusion of the peace of Paris, in 1763, and to give me at the same time an opportunity of receiving the elements of a sounder education, in other respects, than Carolina could afford, he sent me while yet a boy to this kingdom.

In one of his views he was not disappointed. For some time after I had returned to Carolina, to pass a part of my youth under his immediate care, a paper, called AN ASSOCIATION, having been offered for signature to all the male inhabitants of Charlestown above sixteen years of age, the subscribers to which bound themselves to obey implicitly certain authorities unconnected with the former government of the country, I was one of a very few who refused to put their names to it. Those who had now a legal controul over my conduct, my father having shortly before fled from Charlestown to avoid persecution, strongly urged my compliance. They stated, among other things, that many persons of the most undoubted loyalty had signed the ASSOCIATION, and that a continuance in my refusal would expose me to the resentment of the populace. My answer was, that men of established reputation might conceive themselves entitled to a certain latitude of conduct, to which I could not pretend, who had yet a character to gain; and that I was therefore determined, whatever might be the event, that my entrance into manhood should not be marked by what appeared to me an act of treason and rebellion. I was consequently obliged to leave Carolina, altogether uncertain of the future means of subsistence; but I found them here, in the exertions of a father, who, to supply me with what was necessary for the prosecution of my studies, submitted to privations ill befitting his age, and former habits of life. I was in this way enabled to take the degree of doctor of Physic, at Edinburgh, in 1780. Charlestown was now in the possession of his Majesty's forces, and I returned to it for the purpose of collecting the scattered remains of my father's fortune. While there, though exempted by my profession from military calls, I made an offer of my personal services to the commandant of the town, the present lieutenant-general Nesbitt Balfour, and was appointed by him an officer in a body of volunteers, who, by performing a part of the duty of the garrison, enabled a greater number of the regular troops to take the field, than could have done so, without such aid. When Charlestown was abandoned by the king's forces, I went to East Florida. Shortly after my arrival there, apprehensions being entertained for the safety of the province, I requested permission from governor, now general Tonyn, to assist in its defence, and received from him, in consequence, the command of a company of volunteers, who were to serve without pay. This company I raised, and kept together as long as the fears continued, on account of which it had been formed.

I have thus mentioned, my Lord, some of the facts which I possess in proof, that my *conduct* at least was not, formerly, disloyal. They happened at a time of life, from the age of eighteen years to that of twenty-six, when actions are not often discordant with internal feelings; when the veil of hypocrisy is seldom worn, and, if ever assumed, is soon blown aside by the tempests of passion, which so frequently arise in that season of human existence. I shall, however, exhibit more direct testimony that my conduct and principles were in unison. I shall produce to your Lordship a profession of attachment to my country and its constitution, which was made by me in the midst of enemies, from an unwholesome prison, and while threatened with assassination on account of that attachment. For, going to Charlestown, in 1783, upon some family concerns, I was arrested there and thrown into gaol, a few days after my arrival, in violation of a flag of truce with which I had entered the country. Such, at least, was the opinion of governor Tonyn, who had given that flag; for as soon as my arrest was known in Florida, he sent a commissioner to Carolina, Mr. Wyllie, the present chief justice of the Bahama Islands, to demand my release. In the mean time, a publication appeared respecting me, signed by the gaoler in whose custody I had been placed, which began thus; "William Charles Wells, a political sinner of the first magnitude in this land, and now suffering but a very small proportion of those pains and penalties which his high crimes and misdemeanours have so justly deserved, in the common gaol of this metropolis," &c. Nature had not formed, nor had education trained me, to submit with silence to oppression. By means of money, I got a letter inserted in one of the Charlestown newspapers, the following extracts from which will show to your Lordship, whether my sentiments then partook of disloyalty.

Charlestown, in Gaol, July 17, 1783.

"I left this place in August, 1775; purposely to avoid signing a paper, at that time handed about under the title of AN ASSOCIATION. I returned to it in January, 1781, when in possession of the British army, and left it again with those troops in December, 1782. I am, I ever was, and I ever shall be, a subject of Great Britain.

"In what respect, therefore, I can be a 'political sinner of the first magnitude in this land,' and what are those 'high crimes and misdemeanours' which I have committed, I cannot well conceive.——If indeed to wish well to my country while contending with other powers, and to be ready at all times to lay down my life in support of its honour and interests, be a crime, I cheerfully plead guilty to the charge."

"For a freeman to be deprived of his liberty, and lodged in a common gaol; to be kept constantly locked up in a room, whose ceiling is in that condition that the rain pervades it in every shower, sometimes in such quantity that it must be carried out in pails, and whose only window looks to the north, a quarter of the heavens from which the wind never blows when the weather is most sultry, and which not being glazed, obliges him to exclude the cheerful light of day, at the same time that he shuts out the storm*; lastly, to be without the conversation of his friends, whom the dread of popular

* Thunder-storms occur almost daily in South Carolina, in the months of July and August, and almost always proceed from the north or north-west.

resentment prevents from visiting him*; if these sufferings are but a small portion of what

* However unconnected it may appear with the subject of this letter, I cannot forbear mentioning the conduct of two of my friends in Carolina, Mr. John Harleston, and his wife, Mrs. Elizabeth Harleston, persons of rank and fortune in that country. I had received many civilities from them during my stay in Charlestown, while it was a British garrison, and had on my part, done them some small service. But small as this was, it sunk deep into their noble natures, and constituted a debt, unused as they were to receive obligations, which seemed to them inextinguishable. On my return to Charlestown, with the flag of truce, they insisted upon my staying at their house; but it was during my imprisonment that the energy of their friendship was chiefly conspicuous. No one day of the three months which it lasted passed away, without my receiving from them repeated instances of kindness, such as I could have expected only from those, who were bound to me by the closest ties of blood. This conduct would at any time have merited my warmest gratitude; but when I consider the circumstances under which it occurred, my feelings altogether unman me. Mr. Harleston's estate had been heavily amerced by the legislature of South Carolina; and at that period, when the affairs of the state were regulated by the narrow principles of a petty corporation, nothing could tend more to frustrate his hope, that the fine would be taken off, than his showing attention to any one in my situation. The reins of government also were then so feebly held, that the populace almost daily wreaked their vengeance upon such as had fallen under their displeasure. One night, during this anarchy, a mob surrounded Mr. Harleston's house, threatening to destroy it on account of his behaviour to me. He was from home; but his wife, with the spirit and dignity of a Roman matron, went out to the rioters, and told

he is to bear, he can look forward to nothing but DEATH, as the full expiation of his crimes. Grant him but the choice of the mode, and he will thank Heaven for the opportunity of demonstrating his attachment to his sovereign: let but thousands witness that his last prayers were for his country's prosperity, and it will afford him more exquisite happiness in the extreme moments of his life, than good men enjoy when angels sing requiems to their departing souls."

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them, that her husband and herself had done nothing towards me but their duty, and that they should not be prevented from continuing to perform it, by any menace whatsoever. One of those persons is since dead; the other still exists an ornament to her sex. Excellent woman! enjoying in affluence, in the midst of thy children, and their children, the calm evening of a well spent life, and looking forward with a firm hope, inspired by our holy religion, to another and a better state, though thou seemest already to possess as much of happiness, as is compatible with the infirmity of our present natures; it may yet afford thee some momentary satisfaction to know, that neither distance of place, nor intervention of time, hath lessened my sense of thine unspeakable goodness; and that, at this moment, my bosom heaves and my eyes drop tears, while I reflect, that without thy tender cares concerning me, when sick and in prison, and far removed from those, whose duty it was to render me service under such distress, I might long ago have been numbered with the dead.

The smallest drop of blood may become visible on the surface of an animal body, and may serve there some special and useful purpose; sent back to the heart, it is mixed with such a multitude of similar particles, that all marks of it as an individual are lost. In like manner, having returned from the frontiers of the British empire to its capital, I naturally sunk back into the obscurity, which was suitable to my condition in life, rendered now still more low by the poverty, which had been brought upon my family, by their adherence to a great public cause. In more happy times, therefore, than those which have since followed, I could scarcely have expected an opportunity of demonstrating a love for my country, otherwise than by a ready obedience to its laws. In consequence, however, of the attempts which some men, incited to deeds of parricide by the example of successful crimes in a neighbouring state, have made to overthrow our ancient constitution, persons of every rank have within these few years been called upon to declare their attachment to it. I have gladly obeyed this call; and my name appears in the list of those inhabitants of London, who signed the declaration at Merchant Taylors' Hall, in December, 1792; and in that of the same description of persons who signed the declaration at Grocers' Hall, in

December, 1795. More lately, when professions alone were deemed insufficient for the public safety, and a demand was made upon the lovers of their country for their services as its armed defenders, I obtained the honour of being enrolled in a body of men, perhaps not unknown to your Lordship, THE TEMPLE ASSOCIATION, and since I have belonged to it, my exertions to fit myself by a knowledge of military exercises, for the great object of its institution, have not been less than those of many members, younger than myself, and probably not more engaged in other serious pursuits.

It may now appear to your Lordship, that I have spoken of every possible personal objection to my being examined for admission into the college of Physicians. But as pretexts are never wanting to those who wander from the path of honour in search of them, I shall take the liberty of mentioning still another ground, which I have been told they affected to have, for their refusing to inquire into my qualifications. For, Dr. Pitcairn informed me, in the course of last summer, when it could not be foreseen, that he would be unable in the ensuing September to propose me a second time for examination, that, contrary to his former opinion, he now believed that his intended motion would be opposed, on this among other accounts, as he understood, that I had been active in the late dispute between the fellows and licentiates.

That an individual should lose his title to a privilege which had been adjudged by a court of law to belong to a body of men, of which he was a member, merely because he had lent his aid towards obtaining that adjudication, may be perfectly consistent with the notions of right entertained by the college of Physicians, but is certainly not so with those of your Lordship. For if any person had been pre-eminently active in the dispute alluded to, it was surely Dr. Stanger, who, by his applications to the Court of King's Bench, had subjected the college to considerable trouble, expence, and obloquy; and yet your Lordship expressly declared your conviction of his fitness to become a fellow of that corporation. My share in the dispute may be described in a very few words. When it was proposed to me by some licentiates, with whom the scheme originated, to assist in endeavouring to obtain admission into the college by process of law, if it could not otherwise be gained with honour, I immediately consented. I was afterwards appointed one of five to draw up an address to the college, and this address Dr. Cooke, Dr. Stanger, and myself, delivered to the president. These were the only parts of my conduct, in that undertaking, which can be called public, except this appellation should also be given to the subscribing of a small sum of money towards defraying its expence. My private conduct in it was studiously guarded; for as it very soon appeared to me, that the dispute must be terminated by a court of law, I held all private discussion of it with the fellows as useless, and tending only to produce mutual irritation of mind. I therefore constantly forbore to *introduce* it as a subject of conversation, in the presence of a fellow. My reserve upon this point was indeed so strict, that one of that order, with whom I am more intimately connected than with any other physician in London, could not refrain from mentioning it to me, at the same time that he compared my behaviour in this respect with that of another licentiate of his acquaintance, who made the dispute a topic of conversation whenever they met. I mean, however, only to state, not to extenuate my conduct; for had it been as active as that of Dr. Stanger, I should for this very reason have thought it entitled to considerable applause. But I feel ashamed at occupying your Lordship's attention with such trifles. Nothing indeed could have induced me to present them to your notice, but the desire of affording you the most ample grounds for reconsidering the opinion, which you publicly gave of the college of Physicians; and trifles often furnish the most sure, because the most unguarded, avenue to a knowledge of the characters of men.

I HAVE thus, my Lord, endeavoured to prove, that the college of Physicians have not, by their conduct since the decision of the Court of King's Bench, in the case of Dr. Stanger, shown themselves worthy of the high praise, which you were then pleased to bestow upon them. But it appears to me, that if your Lordship had minutely examined the materials of which that body is composed, or had been well acquainted with its previous proceedings, you would not have regarded the honour and good faith of its members, as sufficient barriers against their acting unjustly towards the licentiates, who should apply for admission into their corporation.

In this country, the glory of whose legislators has been to view men as they are found to be by experience, the honour and good faith of no person are, I believe, ever esteemed by the law as adequate securities for his acting justly, when he is tempted to act otherwise by interest. The judges of our superior courts of law are selected from a profession, the conduct of whose members is more open to public inspection, and is consequently better known, than that of the members of any other. No mistake, therefore, can well occur with respect to the characters they possessed before their elevation to the Bench, more especially as few receive that honour before they are past middle age; and every one admits, that, in modern times at least, they have been very generally, if not always, chosen by the executive power with the purest intentions. When they afterwards appear to the world in the exercise of their peculiar functions, the eyes of all men are fixed upon them. Every part of their conduct is scrutinized with the utmost care; by some whom education and habit have particularly fitted for this purpose; by others, whose dearest interests lead them to turn their whole attention to this single point, and whose disappointed hopes naturally suggest some fault in those, who have dissipated their gay dreams, and have awakened them to poverty and disgrace. Yet even these men, so formed to their stations, separated by their retired life from many causes of bias to human opinion, venerated by their country if they act uprightly, detested if they furnish the least suspicion of a contrary conduct, possessing their places by the most certain tenure to persons of honour, receiving for their labours a fixed and ample reward, and solemnly sworn to administer justice impartially, are still supposed liable to be influenced by improper considerations, and are therefore forbidden to try a great class of causes, when these occur in the counties where they were born, or at present reside.

If a situation can be conceived in which interest could furnish no temptation to the abandoning of duty, or none which might not be easily resisted, this would surely occur, when we were charged with the preservation of the life of some one connected with us by the closest ties of consanguinity, who from tender years or imbecility of mind, might be unable to protect himself. On one side, good faith, honour, humanity, the claims of blood, would urge us to the faithful execution of our trust; on the other, public execration, eternal remorse, and disgraceful death, would necessarily present themselves as consequences of its breach. Yet our Saxon ancestors, perhaps not less virtuous than any other nation in the world, whether ancient or modern, building their law upon experience, and knowing hence how unfit men are to resist repeated attacks of interest, where there is the smallest chance that their yielding to them will be concealed, refused to commit an orphan, or person of insane mind, to the care of the next heir, though he were the nearest relation.

It would, I think, be difficult, if not impossible, to point out, in any part of the world, a large body of men, who are more likely, in their collective capacity, to regulate their conduct by the principles of honour and good faith, than the Commons of the Parliament of Great Britain; and yet not many years have elapsed, since they confessed by their proceedings, that they had often corruptly exercised the power of determining contested elections to their House, and by a noble act of general justice, deprived themselves of the means in future of violating the rules of right in detail.

Distrust of the virtue of mankind, seems indeed to be a leading principle of the constitution of our country. The supreme power of the state is vested in no one person, or set of persons; but is broken down into various parts, which are distributed among different descriptions of men. Each of these, from the original laws of human nature, aims at its own aggrandizement, which the others labour equally to oppose. From this contention arises the most lovely order; our public happiness is thus bottomed in our private infirmities, and the stability of our government is secured by the very means, which to superficial observers appear fraught with its destruction.

If therefore it cannot be inferred from the common qualities of Englishmen, that the college of Physicians, when under no other controul than that of honour and good faith, will always act justly, it appears to me that, setting aside actual experience, the only ground for expecting such conduct from them must be looked for in the habits and principles, which physicians acquire in the practice of their profession. The probability of finding it there shall be my next subject of inquiry. This perhaps will be best conducted by considering, in the first place, the state and estimation of medicine, when exercised as a gainful art, in ages and countries different from our own.

When men first begin in any country to practise the medical art for hire, their knowledge of diseases, and of the proper modes of treating them, is necessarily very small. To conceal, therefore, their ignorance, they affect mystery, and have recourse to various modes of deception. Thus, in all rude nations, physicians have pretended to use supernatural means in the cure of diseases; among those nations indeed, the different trades of conjurer and physician are commonly exercised by the same person. But such a course of life must debase the character, in every respect, of him who follows it. No one can promise to himself, that he will stop at any certain point in villany. Temptation solicits him to proceed, and his powers of resistance diminish as he advances; till at length he arrives where honesty and truth seem no more than scare-crows, set up by designing men to prevent the weak and timid from pursuing their own good.

As the knowledge of diseases and their remedies increases, the obtaining of it becomes more difficult, and from the general progress of improvement, there are now men who can estimate the value of the acquisition. Physicians are therefore less tempted either to conceal their methods of cure, or to pretend to derive assistance from supernatural agents. Hence medicine, considered as a gainful profession, has for the most part been less despised in civilized, than in barbarous nations. It appears, however, to have been held in very little estimation, even by the most polished nations of antiquity, of which we have any tolerably well authenticated accounts.

In Egypt, a physician, who attempted to cure a disease by means different from those which were mentioned in the sacred books, forfeited his own life, if his patient died. By the confession of Hippocrates, medicine was regarded by the Greeks as the lowest of the arts. The oath which he exacted from his scholars, not to commit some of the vilest crimes, and to keep secret the knowledge which he should communicate to them, is a strong proof of the truth of his observation. With the Greek comic writers, "a son of Hippocrates," was a term of derision. So low indeed was the condition of physicians in Greece, that Alexander the Great seems to have been neither affected with remorse, nor accused of cruelty, for crucifying Glaucus, the physician of Hephæstion, though the death of his favourite had been occasioned by his own imprudence. Many learned men have shown that, before Julius Cæsar, the physicians in Rome were, for the most part, if not altogether, either freedmen or slaves. Afterwards, medicine rose there somewhat in esteem, both from the greater knowledge of its professors, and the degradation of the former civil distinctions in society; but it was still attended with so little respect, that even Galen was afraid to prescribe some pepper in wine to Marcus

Aurelius, for a pain in his stomach, because it was too strong a remedy for an emperor.

It forms no argument against the justness of this statement, either that kings and princes anciently exercised the medical art, or that physicians were sometimes held in considerable estimation by the great. For, in the first place, there are many arts which adorn those who cultivate them for their own use or amusement, or for the benefit of others, but which degrade the persons that practise them for money. Our country gentlemen are very desirous of knowing the diseases of horses, and their remedies: but the trade of a farrier is with us a very low one. The talent of singing is much prized by females of the highest rank; yet how meanly are those persons thought of, who gain by it their livelihood? And secondly, eunuchs, and other men confessedly of the vilest condition, have not unfrequently been entrusted with the management of empires.

Physicians have, in modern Europe, obtained a higher rank in society, than they possessed among the ancients, principally however, as it appears to me, by means entirely unconnected with the exercise of their profession. For, upon the revival of a taste for letters in our western parts of the world, some persons applied themselves to the study of the ancient writers upon medicine, with the view of becoming more successful practitioners of that art, than those were, who had learned it in the ordinary manner. But the same skill in languages, which was necessary for this undertaking, fitted them also for the acquisition of every other kind of knowledge, which had been treated of by the authors of Greece and Rome. They made use of this advantage, and physicians became noted for their proficiency in every branch of the learning of antiquity. This erudition naturally rendered those who possessed it respectable, and, by an obvious association, raised their profession in the esteem of the public. It produced the same effect in another way. A tedious and even expensive education was henceforward deemed requisite for physicians, which could be borne only by persons of some fortune, and therefore, less likely to be guilty of baseness and deceit, than men in the low condition of the former practitioners of medicine.

The operation of these causes was, in this country, considerably assisted by the same circumstances, that have given our merchants and manufacturers their present place in society; and by reason of this combination, its physicians hold a much more elevated situation than those of any other considerable nation in the world. When an English physician travels upon the continent of Europe, he frequently finds that his profession, if known, is a bar to his reception into good company, and therefore very generally conceals it.

But, my Lord, though the physicians in this country have been thus freed from, what may almost be termed, the necessity, which formerly existed for using improper means to gain employment, they are still often strongly tempted to do wrong in the same pursuit. They are, indeed so often, and so strongly tempted to do so, and are at the same time, from the nature of their profession, so little liable to be prevented from yielding, by that great guardian of virtue, public censure, that it seems to me beyond a doubt, that the body of physicians here must contain a greater proportion of persons, who have made undue sacrifices to their rise in the world, than several other classes of Englishmen; than, for instance, the body of barristers, with which alone, indeed, it can properly be compared. What knowledge I have of this subject, is derived from my residence in London; the observations, therefore, which I shall make upon it are, in strictness, only applicable to the state of physicians in the capital. Your Lordship, however, will not suppose it my intention to insinuate, that I have not yielded to the same temptations: *Video meliora proboque; deteriora sequor.* A soldier may relate the defeats, as well as the victories, in which he has borne a share.

The young men, who apply to the study of medicine in this country, are chiefly of small original fortune, and the greater part of this is commonly consumed in their education. Very few physicians, therefore, when they come to London to exercise their profession, which, if they have graduated at either of the English universities, they seldom do till they are nearly thirty years old, have sufficient incomes for living in the manner, which is thought here becoming the rank of a gentleman. They are consequently extremely desirous to supply this deficiency in their private fortunes by the profits of practice, and their age strongly urges them against every needless delay in attempting to accomplish this end. Barristers, from entering more early into their profession, may with less inconvenience wait the gradual approach of business. These too have frequently, soon after they commence practice, opportunities of appealing to the world, in the most honourable manner, on their fitness to be employed. They address themselves publicly to men well qualified to judge of their abilities, and upon subjects of which almost every person understands as much, as renders him capable of determining, whether or not they have been rightly conducted. If the exhibition of talents has been considerable, it is soon very generally known, and is in a short time followed by an increase of employment, from the desire of many to benefit themselves by their assistance. A physician has no such opportunity of showing the knowledge which he possesses; he possesses indeed, on beginning practice, much less knowledge capable of being turned to immediate use, than a barrister of the same standing, and equal application. His art is founded upon experiment and observation, and the rules for exercising it are always modified by external circumstances, which can never be accurately known, except by one long conversant with diseases, as they actually occur. Skill in medicine is therefore not to be acquired by reading alone: whereas law, being a collection of the opinions and ordinances of men, is necessarily studied in books; and hence a considerable knowledge of it may be obtained by those, who have seen little of its application to particular cases. Besides, a young barrister does not appear in the management of any case, until a considerable time has been spent by him in preparation; but the first calls upon a young physician are frequently to oppose sudden attacks of disease, which do not permit his thinking long, how this can best be done. For these reasons, it seldom happens, that physicians either merit much praise from their first efforts to cure diseases, or quickly acquire a considerable increase of practice from any single display of great talents. They must consequently be strongly disposed to adopt other means to raise themselves to notice.

The present possession of practice being a considerable recommendation of physicians to further employment, every young physician finds an advantage in having it thought, that his business is greater than it actually is; and should he endeavour to impress the public with such an opinion, the privacy with which the medical profession is for the most part exercised, prevents any flagrant discovery, that it is not well founded. Many of them are therefore induced, notwithstanding the smallness of their incomes, to imitate the exterior expence of their seniors, hoping that the world will hence believe, that they enjoy a corresponding degree of employment. The business of a barrister being, on the contrary, chiefly conducted in open courts, any attempt to make it appear greater, than it is in reality, would soon expose him to ridicule and disgrace. He lives, therefore, except his private fortune be large, for many years in Chambers, and goes to Westminster Hall in a hackney-coach; whereas a physician, sometimes immediately upon coming to town, very commonly only a year or two after, occupies a whole house, and visits patients in his own chariot. But this expence, though its object should be ultimately attained, reacts in the mean time upon the cause which gave rise to it, and augments in him the necessity for professional gains.

The female sex, it is well known, have great influence on the extent of practice which physicians possess. But, for many reasons, they are averse to communicate their own complaints to any one who is unmarried, and they naturally recommend to others the person whom they consult themselves. Physicians, therefore, very generally marry soon after they commence practice. As they are then far from being wealthy, if they marry women in other respects equal to themselves, they seldom receive fortunes with them. In this case, the calls for money increase, for some time at least, more rapidly than the beneficial effects of their new situation; and hence, actions, which were formerly regarded as contemptible, will now perhaps seem even praiseworthy, from affording subsistence to the objects of their most tender affections. If, on the other hand, they marry rich women, these are commonly unequal to them in some material circumstance, in age, education, habits, or personal appearance. But a sacrifice to interest, in so momentous a concern, is surely no pledge, that they will not make others of less importance, in the exercise of their profession. Barristers are much less exposed to this cause of ill conduct in the pursuit of employment. Marriage gives to them no advantage in it; and hence, they generally either enter into that state later in life than physicians, or remain single to the end of it.

What I have said, my Lord, seems sufficient to show, that the physicians of London are often placed in situations, in which temptations to do mean things for money are known by experience to act forcibly. But collections of men appear to be more or less virtuous, nearly in proportion to the number and greatness of the enticements to vice, with which they are surrounded. The principles of honour may, indeed, become more firmly fixed in the bosoms of some few individuals of uncommon make, from the very attempts which are made to loosen their hold; but though gold is purified and brightened by fire, common metals are by the same agent turned into dross. According to the model of prayer, which has been given to us by the divine author of our religion, we are not to petition for strength to resist temptation; man's presumptuous confidence in his own powers might have been heightened by such a permission: but we are humbly to beg our heavenly father not to *lead* us into it, hereby confessing our insufficiency for the contest, whenever it shall occur.

I do not, however, my Lord, wish to convey an opinion, that physicians become dishonest in the situations which I have described; my design is fully answered, if I have rendered it probable, by stating the difficulties in which they are frequently involved, that their temptations to lay aside the character of men of high honour, are sometimes too great for resistance. I now add, that proofs of their actually yielding to those temptations are furnished by what we daily hear of their needless visits to sick persons, their rapacity with respect to fees, and their servility to apothecaries*. When these, or similar

* The present division of medical practice in this country, between physicians and apothecaries, did not commence in London, until some time after the separation of the latter from the grocers, in 1617, and was not firmly established, before the great plague in 1665, during which, by far the greater part of the physicians having fled into the country, the apothecaries were left with almost the entire care of the sick. These facts were at least advanced in a controversy, which existed about the end of the last century, respecting the title of apothecaries to practise medicine, and were not

practices have been adopted, they are not often afterwards abandoned, because the circumstances

then contradicted. To support them, it may be mentioned, that according to a publication from the college, dated 1698, the number of apothecaries in London and Westminster, sixty years before, was not 100, but was then above 800; and that in 1701, they were said to be nearly 1000, partners included. At the date of their charter, in 1617, the number was 114; so that it must have decreased for the first 20 years after their separation. This division, however, seems to have begun more early in some other parts of the kingdom; for a physician of Salisbury speaks of it in 1566 as being lately introduced there. Its origin may, I think, be placed in the greatness of the fees, which English physicians have always been accustomed to receive. I find many notices of an angel, or ten shillings, being the usual fee to them, from 1665, to the beginning of the present century; and in 1670, Dr. Goddard, a fellow of the college, and Gresham Professor of Physic, asserted, that the fees then given were according to the ordinary and accustomed rates, time out of mind in England. Many persons, therefore, who wished to receive benefit from medicine, but unable or unwilling to fee physicians so largely, and at the same time too proud to solicit their gratuitous aid, would naturally apply to those, who offered both advice and medicines at a cheap rate. This also seems the chief reason, and not the greater credulity of the people, why empirics formerly abounded here, more than in any other country in Europe. For, since the complete establishment of apothecaries, as medical practitioners, the number of empirics has been considerably lessened; the descriptions of men, who on account of cheapness used to resort to the latter, now applying to the former, for the cure of their complaints. The existence then of a lower order of practitioners of medicine appears necessary in this country; and which gave them origin have ceased. The pride and delicacy of a gentleman, if once

the attempts of the college to destroy it were as absurd and unjust, as they were fruitless.

When the division first took place, one of its effects was probably not foreseen. For apothecaries coming at length to be employed by many persons who were sufficiently rich to fee a physician; when the assistance of one was desired by these in dangerous disorders, the choice of the individual was frequently left to the apothecary, he being supposed better qualified to make it, than the sick person or his family. From this time, therefore, the friendship of apothecaries became highly useful to physicians, and was often sought for, and requited by them, in the most disgraceful manner. I might bring many proofs of these points from authors of the last century; but I shall content myself with one, the authenticity of which is beyond doubt, as it is found in an account of the proceedings of the college, in establishing a dispensary for the relief of the sick poor, which was published by themselves, in 1697. They there say; " Several amongst them [the apothecaries] set themselves by all the art and industry they were capable of to frustrate the whole design; and finding no method so promising, as to stir up a party among ourselves, to oppose our proceedings, they fell to intriguing with several of our own members, *who were too easily lured off to serve the apothecaries' interest, for their own private advantage.* And from this cause, as we have too much reason to believe, have chiefly sprung the unhappy differences, which are still fomented among us. But notwithstanding all the discouragements we met with from those of our own members, who *contrary to all the obligations of honour and conscience,* constantly discovered to our adversaries whatsoever passed in the college relating to this design, and exposed to them the names of such as were promoters thereof, surrendered, are scarcely, I fear, ever fully regained. No one, however, who does not completely possess them, is surely fit to constitute a part of *the sanctuary of honour and good faith.*

that they might be kept out, as far as in them lay, from all patients where they should be proposed, and themselves brought in," &c. The college of Physicians, therefore, a hundred years ago, were surely not *the sanctuary of honour and good faith*; since one part of them were then declared by their colleagues to have violated every obligation of honour and conscience in pursuit of their private interest; while those, who had thus erected themselves into censors of morals, openly confessed, that they were afraid to have it known they were doing a right thing, lest they should not be called in by apothecaries to see their patients. Physicians, in general, have in the course of the present century become more prudent, and, I believe, more honourable; but it is, notwithstanding, very notorious, that many of them at present cultivate the acquaintance of apothecaries, in ways very disreputable to gentlemen. Barristers may be tempted, though, I think in a less degree, for reasons already mentioned, to act similarly towards attornies; but the restraints upon their yielding, are much greater. Their frequent meetings in courts, and upon circuits, afford many opportunities of discovering defaulters, and of inflicting punishments, which few are hardy enough to disregard; whereas physicians, having little necessary intercourse with each other, are consequently in a great measure without the salutary fear of the reprehension of their equals. In what estimation would a barrister be held, who should give frequent and costly dinners to attornies? But it is said, and I believe truly, that physicians of great eminence have derived much of their practice from giving such dinners to apothecaries.

But there are various circumstances in the practice of medicine, unconnected with its profits, which tend to injure the character of those who follow it. An action at law remains at rest, except it be urged forward by human force, and its termination is induced by means, which we can easily comprehend. The value, therefore, of the talents employed by any one in conducting it, may be tolerably well appreciated, and the fame which hence arises to him, is almost always proportioned to his merit. It is far otherwise in medicine. Diseases proceed by their own energy, and terminate spontaneously, for the most part, in health. Such a termination, however, of a dangerous disease, if a physician has been concerned in its management, is very commonly attributed to his skill. He may at first blush at undeserved praise. At length, from frequent repetitions of it, he often fancies himself really capable of producing the effects, which he hears attributed to his agency. Again; should a barrister have any natural tendency to overrate his talents, the frequent mortifications he must experience, in his daily contests with others of his own class, before public assemblies of men, will soon teach him to value them more justly. The same corrective is not applied to physicians. In the exercise of their profession, they appear always as dictators of rules to others, and the feeling of self-importance, which this situation excites, in time often diffuses itself over every part of their conduct. Men too form insensibly an estimate of their own worth, from secretly comparing themselves with those whom they see most commonly. But well-employed physicians spend much of their time in the company of persons weakened in mind by disease, and of the female attendants of sick rooms; it ought not then to seem strange, if, like schoolmasters from conversing chiefly with children, they should acquire an opinion of their own talents, much higher than what they merit.

I shall take notice of only one other source of injury to the character of physicians. Those among them of the greatest learning and experience know well, that the most unexpected changes sometimes take place in diseases, and are best acquainted with the difficulty of referring to their proper causes, the various events that occur in so complicated a structure as the human body. It might therefore be thought, that such men would always be modest, cautious, and even timid, in the practice of their art. But this is not the conduct which recommends a physician most. It suggests to a sick person, what indeed may be true, that a doubt exists respecting the nature of his complaints, than which nothing can be more distressing to him. He often, therefore, applies to one, who acknowledges no difficulty in the treatment of diseases, who pretends to see clearly what is hidden from human beings, and who speaks of uncertain events, as if they were entirely under his command. In this way, the sick man is gratified, but too frequently at the expence to the physician of one of the most valuable parts of the character of a gentleman, and faithful observer of nature. The exquisite painting by Moliere of the vanity, affectation, and pedantry of the French physicians of his time, exhibits a resemblance to the general character even of English physicians of the present day, which is sufficiently strong to make it probable, that those qualities are, in a greater or less degree, almost inseparably connected with the exercise of the medical profession. But he in whom they exist, though he should have the most upright intentions, will often decide as unjustly, when his own interest or consequence in the world is concerned, as if he had been actuated by the vilest motives. Before men, who are not governed by others, can do what is right, they must first clearly perceive it, which nothing certainly more effectually prevents, in whatever has relation to themselves, than a false or extravagant opinion of their own worth.

Many of our physicians have no doubt received little injury from the causes of the corruption of character, to which they have been exposed; and some few may have escaped their influence altogether. One of these few, Dr. William Heberden, I must conclude to have been well known to your Lordship, from the eulogy which you pronounced upon him, during the trial of Dr. Stanger's cause. He was probably, indeed, the only physician with whom you were intimately acquainted, and hence, from the natural error of attributing to a whole species the properties of its only individual we have seen, you might imagine, that he possessed his many virtues in common with the rest of his class. But Dr. Heberden, my Lord, stands, in a manner, alone in his profession. No other person, I believe, either in this or any other country, has ever exercised the art of medicine with the same dignity, or has contributed so much to raise it in the estimation of mankind. A contemplation of his excellencies therefore can afford little help towards obtaining a just notion of the general worth of physicians. In speaking of a mole-hill, we would not employ terms that had relation to the immensity of a mountain.

Were I, my Lord, possessed of talents adequate to the undertaking, I should here endeavour to describe at full length the character of that illustrious man. In this attempt, I should first mark his various and extensive learning, his modesty in the use of it, and his philosophical distrust of human opinions in science, however sanctioned by time, or the authority of great names. I should then exhibit him in the exercise of his profession, without envy or jealousy; too proud to court employment, yet undervaluing his services after they were performed; unwearied, even when a veteran in his art, in ascertaining the minutest circumstances of the sick, who placed themselves under his care, taking nothing in their situation for granted, that might be learned by inquiry, and trusting nothing of importance that concerned them to his memory. To demonstrate his greatness of mind, I should next mention his repeatedly declining to accept those offices of honour and profit at the British court, which are regarded by other physicians as objects of their highest ambition, and are therefore sought by them with the utmost assiduity. I should afterwards take notice of his simple yet dignified manners, his piety to God, his love for his country, and his exemplary discharge of the duties of all the private relations in which he stood to society; and I should conclude by observing, that his whole life had been regulated by the most exquisite prudence, by means of which his other virtues were rendered more conspicuous and useful, and whatever failings, he might as a human being possess, were either shaded or altogether concealed. After my description was finished, I should think it proper to say, that I had never been acquainted with Dr. Heberden, and consequently could neither be dazzled by the splendour of his virtues, from approaching them too nearly, nor influenced in my opinion concerning them, by benefits he had already conferred upon me; and that standing, as he does, upon the verge of this state of existence, ready to wing his flight to another of glory, his ear must now be closed to the voice of flattery, had he ever listened to that siren, or were I base enough to solicit her aid, in the foolish expectation of receiving from him some future reward.

I think, my Lord, it has now been shown, that physicians, considered singly, cannot by reason of the discipline of their profession, claim exemption from the moral infirmities, to which the other inhabitants of this country are subject. Is it then to be supposed, that a *body* of them will always be governed by the strictest rules of justice? Is it, my Lord, at all consistent with the experience we have of human actions to expect, that those, who may have individually yielded to temptations of interest, will, when exposed in a collected state to similar temptations, continue long to deserve the title of *the sanctuary of honour and good faith?*

But perhaps it will be said here: "Granting that the college of Physicians, like other men, are open to the influence of motives, which pervert or corrupt the judgment, it is yet impossible not to believe, that their general conduct is agreeable to the common maxims of prudence. Their reputations must surely be dear to them; these therefore they will not hazard, without the prospect of some advantage to compensate the risk. But with respect to the admission of licentiates into their body, the circumstance which has given birth to the whole of this discussion, what *interest* have they in acting unjustly? Unless then it shall be clearly established, that they have such an interest, the attempts which have been made by the author of this letter to depreciate their character, must be regarded as the offspring of spleen or disappointed ambition, to bestow upon them no harsher appellation." Anticipating, my Lord, these observations, I proceed to reply to them. In doing this, I shall be led to the last purpose of my address, namely, to present to your Lordship's view, several proceedings of the college, *previous* to the decision of the Court of King's

Bench in Dr. Stanger's case, which, if known or minutely considered by you, might have possibly induced an opinion respecting the integrity of their corporate conduct, far different from what you then so warmly expressed.

In the first place, it will be scarcely denied by any one, in the least acquainted with medicine as a practical art in London, that physicians conceive it of much importance to be fellows of the college. This indeed seems sufficiently proved, both by the eagerness with which admission into the fellowship has been sought by some of our most celebrated physicians, Hunter, Fothergill, and Fordyce, not to mention other and later names, and by the obstinacy with which their endeavours to gain it have been resisted, by those already in possession of the corporation. It will not diminish the force of this argument to assert, that the object in dispute was altogether unworthy of the exertions, to which it gave rise. Men do not always estimate the value of things, either according to the profit they produce, or by the rules which may possibly guide the opinions of superior beings. What more trite, and, at first sight, more just subject of ridicule is there, than the vehement desire which many exhibit, for the possession of a piece of ribbon of a particular colour? Yet this desire exists with persons of the first talents, fortune, and rank in this country:

> "Let school-taught pride dissemble all it can,
> "These little things are great to little man."

Though it be unnecessary, therefore, to proceed further in proving the value of a fellowship of the college, I shall, notwithstanding, briefly mention some of the advantages, which accrue to physicians from possessing it.

There are various offices, lectureships, and appointments in the college, which are attended with profit, and are filled by fellows alone. The emoluments of these, though not considerable, are still of sufficient magnitude to render them objects of desire to physicians in the first years of their residence in London; and hence, as I have been informed, they are frequently given to the younger fellows, with the view of assisting them during that difficult period.

The chief advantages, however, which a physician enjoys from a fellowship of the college, are in consequence of his being often placed by it, in very conspicuous and honourable situations. Soon after receiving it, he becomes an examiner of the fitness of other physicians to be fellows or licentiates; a visitor of the shops of apothecaries, for the purpose of inspecting the quality of their medicines; and a commissioner, under an act of the legislature, for licensing houses for the reception of lunatics. By these means, though he may be a very young physician, he nevertheless appears to the world as a man of rank in his profession. Such a circumstance to the greater part of persons must be highly gratifying, without regard to its consequences. But in medicine, the slightest sign of distinction is frequently a source of profit to the possessor; for as men, in general, have not sufficient knowledge or discernment to choose their physicians on the ground of merit, they commonly take those who exhibit marks of public approbation and confidence. A fellowship, therefore, by bestowing such marks, is often greatly conducive to the advancement of the interests of a physician. It is far indeed from always happening, that fellows of the college rise to eminence, as practitioners of medicine; but the fact is undoubted, that they rise to it more frequently and more quickly, than licentiates in every respect equal to themselves, except as to the relation in which they stand to the college.

But it is evident that these, and all other advantages of a fellowship, will be more or less amply enjoyed by individuals, according as few or many are entitled to partake of them.

Whether any body of men would be able to resist such a temptation to restrain the increase of their number, I know not. It is certain, at least, that the college have not been so, but have often adopted measures for this purpose, which are declared, by persons of the highest authority, to have been contrary to the laws of our country. "Licences," said Lord Mansfield, while delivering a judicial opinion upon the conduct of that corporation, "probably took their rise from that *illegal* by-law, now at an end, which restrained the number of fellows to twenty. This was *arbitrary and unjustifiable*; they were *obliged* to admit all such as came within the terms of their charter."

The effect, which was once derived from restraining by-laws, is now produced by means less odious in appearance, but not less sure in operation. Though a degree of doctor in medicine, from Oxford or Cambridge, has been demanded by the college, almost from its foundation, as a qualification for a fellowship; yet, for a considerable time, it was occasionally dispensed with, and when it was not, physicians, who had graduated elsewhere, could for a small sum of money, readily procure such a degree from those universities, by incorporation *. But,

* The degrees, which students of Oxford and Cambridge receive from their own universities, are conferred by *creation*;

towards the end of the last century, laws were passed by our universities, at the desire, it is said, of the college, to prevent in future the incorporation in them of physicians, who had graduated in any place out of England, except Dublin; and since then, the college have never, I believe, admitted any one to an examination for a fellowship, who did not possess an English degree of doctor in medicine. The consequence has been, that the number of members, which in 1677 was sixty-five *, without including twenty *honorary* fellows, a class no longer existing, is now only forty-eight †, notwithstanding the vast increase, which the capital has in the mean time received, in point both of population and riches. But all surprise at this diminution of the number of members will cease, when it is known, how greatly that of licentiates has during the same interval been augmented. In 1667,

but when a graduate from a different university is admitted in either of them, *ad eundem gradum*, this is called *incorporation*.

* Fifty-three fellows and twelve candidates, who are both, in the language of the college, named *collegæ*. The term *candidate* is used in a very different sense by the college from what is commonly given to it; with them it means a person who has passed all the examinations which are required for a fellowship, but who is not actually in possession of it. I have for this reason very seldom employed it.

† Forty-five fellows and three candidates.

there were only ten persons in that class; while the present college list contains one hundred and five, the far greater part of whom would have been admitted as fellows, if the English universities had not repealed their former laws for granting degrees by incorporation.

The system of admission which has produced these effects, is that which the college, after being repeatedly admonished of its narrowness and injustice by Lord Mansfield, professed to amend, by the two by-laws already so often spoken of. That they have an interest, however, directly contrary to the pretended object of the new laws, is clear from the tardiness alone with which these were brought forward. Lord Mansfield began in 1767 to censure the old laws of admission, yet the new were not made before 1778*. The succeeding history of one of the latter demonstrates the existence

* The college, during the trials of Dr. Stanger's case, seemed to have been much ashamed of the dates of these laws. They were not mentioned in Mr. Roberts's affidavit, and when asked for by the judges, the counsel of the college appeared ignorant of them. If the omission had not been by design, they would surely have been inserted in Dr. Gisborne's affidavit in answer to Dr. Stanger's second application; but upon this subject he was equally silent with Mr. Roberts. At length, after repeated questions from the judges during the second trial also, it was extracted from Mr. Dampier, that the new laws were made in 1778.

of the same interest still more strongly. This at first authorized the introduction, by favour, of two licentiates every year into the college. But it was quickly after enacted, that only one should be annually proposed for introduction; and again, that no proposition of this kind should be made oftener than once in two years. Such are the changes which the *letter* of the law has undergone. If we look to its execution, it may now be regarded as abrogated; since no licentiate has been proposed under it for six years past.

But, though the college have thus shown, that they possess a strong interest in preventing the increase of their number, from the introduction of licentiates by favour, it is yet easy to prove, that they must have a much more powerful one, in resisting the entrance of persons of that class, through the means of examination. Licentiates made fellows in the former way will naturally adopt the maxims of their patrons, with respect to the management of the corporation; and even if they should not, they can never be sufficiently numerous to form in it a party of any consequence. On the other hand, licentiates admitted to be fellows of the college, after an examination of their fitness, would be free to act in all its concerns, according to their own views of what was right. They might consequently dispute both the justice and expediency of acknowledging in the graduates of Oxford and Cambridge, any title to be received into the corporation, which does not depend upon their learning and good character; and their own number might in a few years become so great, as to exceed that of all the other resident fellows. Can we now even imagine, that the present fellows of the college, all of them, except five persons who have been admitted through favour, physicians from Oxford and Cambridge, are not generally hostile to a measure, which, if executed, must immediately diminish some of their own advantages, and may hereafter deprive the members of the English universities of the chief rule in a corporation, which has long been regarded by them as their own?

I have thus, my Lord, replied, and I hope satisfactorily, to the question concerning the interest, which the college have in acting unjustly towards those licentiates, who may apply to them to be examined for fellowships; and, while doing this, I have proved by indubitable testimony, that even before the decision of Dr. Stanger's case, they had not always shaped their conduct by the rules of honour and good faith. It may therefore be thought, that my address to your Lordship ought now to close, since its various objects have been attained. But, as in my opinion, it deserves to be still further considered, whether an accurate knowledge or estimation of some preceding acts of the college might not possibly have produced a doubt in your Lordship's mind, on the propriety of surrendering to them the sole determination of claims, which they have various and manifest temptations to determine unjustly, I shall venture to trespass a little longer upon your Lordship's patience, by offering a few additional observations upon this part of my subject.

The first I shall make is derived from a circumstance in the general conduct of the college, of which your Lordship took notice, when you delivered your opinion upon Dr. Stanger's second application. On that occasion your Lordship said: "By what fatality it is, that almost since this charter has been granted, this learned body has somehow or other lived in a course of litigation, I know not; one is rather surprised, when one considers, that the several members of this body, including the licentiates, the commonalty of this corporation, are very learned men: and as much as it is not generally the fruits of learning, at least not the best fruits of learning, to get into litigation, one cannot tell how those learned gentlemen have fallen into so much litigation." The fact here mentioned, though highly important, may not to many, however, appear so surprising as it did to your Lordship. Learned occupations, by withholding their followers, for the most part, from the busy paths of life, necessarily exempt them from many occasions of dispute, to which other persons are exposed; but few are more ready, than literary men, to embrace such occasions of dispute as are presented to them. In whatever regards the fruits of their mental labours, this is universally acknowledged to be true; the title of *genus irritabile*, though more especially given to poets, is found to be applicable, in a greater or less degree, to every description of authors. Some of the malevolent passions, indeed, frequently become in learned men more than ordinarily strong, from want of that restraint upon their excitement which society imposes. Perhaps too, from a well-known law of human nature, their moral feelings may be less correct than those of many other men, in consequence of the great and frequent exercise, which is given to the powers of their understandings. Physicians, therefore, as men of learning, have their causes of dissension with each other; as men seeking wealth by their learning, or affectation of learning, they have many more. The great bulk of mankind being unable to judge of the truth of their dogmas, or the propriety of their practices, it is very natural, that a number of them should jointly endeavour to persuade their sovereign, that they are the only fit persons to take care of the health of his subjects; while in truth, the great object of their combination is to establish a monopoly of medical employment in their own favour. This I believe to be the real origin of our college of Physicians, notwithstanding the praises which have been lavished upon its founders. Its charter was granted in the age of monopolies, when men of much higher rank, and greater private respectability than physicians, were eager to obtain them. Some surgeons procured, about the same time, a monopoly of their profession in London; but being less wary than the physicians, or the operations of their art being more subject to the examination of the external senses, they were shortly after declared by an act of Parliament to have abused their trust most grossly. Though the college have not experienced a similar disgrace, the defence of their monopoly has yet involved them in that constant course of litigation, which has so much excited your Lordship's surprise. But had your Lordship advanced a single step further in this subject, it would certainly, I think, have occurred to you, that the members of a body, which for nearly three hundred years had been almost constantly engaged in law-suits, were not very fit persons to be entrusted with the power of deciding on the claims of those, whom it was their interest to depress. The frequent appearance of men in our courts of law, whether as plaintiffs or defendants, is not, I believe, generally held such a proof of their virtue, that they are hence thought capable of exertions of self-denial, to which others of a more retired life are acknowledged to be unequal.

Possibly another source of doubt, respecting the fitness of the college to execute with fidelity so difficult a trust, without the inspection or controul of some superior power, would have been furnished to your Lordship, by a comparison of the circumstances, which precede and attend the admission among them of the two descriptions of men, who are entitled to apply for it. A physician of Oxford or Cambridge, who possesses a desire to enter the corporation, has no obstacle to fear to its completion, from any general prejudice against him in the minds of those who are already members. He has, on the contrary, reason to expect, that he will be received by the body at large with pleasure, both because he comes from one of their own universities, and has completed there the course of study, which they regard as by far the most proper to form a physician, and because his admission will tend to prevent the necessity of their adopting persons of a different education, to render their number sufficient for the customary rotation of corporate offices. Nor can any of the members well entertain a personal dislike to him, as he has scarcely yet begun to contend with them for employment. Under these circumstances he applies to the college, at any time he finds convenient, for an examination of his qualifications, which is immediately granted as a matter of course. The examination is delegated to the president and the four censors, who are all chosen to their offices for only a year, and, to use the language of the college, "are strictly sworn to do justice." It is divided by them into three parts, each of which is held at one of their separate meetings*, and their decision upon his fitness is seldom or never formed, until he has been subjected to all the parts. Should the decision be in his favour, at the next general meeting of

* I know that the president and censors may hold the examination, if they please, at the general meetings of the college; but no instance of their doing so has, I believe, occurred for many years, and if they were to hold it at those meetings, none except themselves would have a title to determine on the fitness of the person examined.

the college he is proposed for admission. A ballot is then taken, and if a majority of the votes be in support of the proposal, he becomes a member of the corporation, with the title of *candidate*. The whole of these proceedings, including the original application, are sometimes finished in a week or two, and always in less than three months. After he has been a candidate for twelve months, without further examination, and almost without further ceremony, he is received into the order of fellows. If he has come to London shortly after obtaining a doctor's degree, his admission into the fellowship almost always takes place, either before or about the thirtieth year of his age.

I turn now, my Lord, to the licentiate who is engaged in a similar attempt. Though the college, from deference to the authority of Lord Mansfield, have apparently ceased to view an English degree, as an indispensable part of the title of a physician to be examined for a fellowship, the prejudices* and interests, which

* Some notion may be formed of the extent of these prejudices, from the undermentioned circumstances in the conduct of Sir Lucas Pepys, as physician general to the army. I possess indeed a still more flagrant example of their influence; but I prefer the present, as being of a public nature.

Suspicions having arisen in the beginning of the present war, that the dreadful mortality of our troops in the West dictated their former laws of admission, still exist with undiminished force. Whenever,

Indies, had, in part at least, been owing to their want of proper medical aid, it necessarily became an object of great national concern, that the immense armament, which was preparing, in 1795, to be sent to those countries under the command of Sir Ralph Abercrombie, should be provided with able physicians. In this state of things, Dr. William Wright of Edinburgh was mentioned to a person in power, as being well acquainted with the diseases of the West Indies; in consequence of which, a gentleman, connected with administration, authorised a common friend to make him the offer of being a physician to the armament. Having signified his willingness to accept this appointment, he was desired to remain in Edinburgh, until his services should be required.

It is proper to say somewhat here concerning the fitness of Dr. Wright, for the situation to which he was designed. *He was a fellow of the college of Physicians of Edinburgh*; and had formerly served his Majesty seventeen years, chiefly in the West Indies. He had, besides, practised medicine in Jamaica, while unconnected with the army, for thirteen years, during great part of which time he was Physician General to the militia of the island. His talents had not, in the mean while, been confined to the cultivation of the practical part of his profession. Having included natural history among the objects of his study, he had, during his residence in Jamaica, explored almost the whole of it, in his attempts to extend the limits of that science, and had in consequence made many important discoveries of plants, some of which had been published in the Philosophical Transactions of London and Edinburgh, and various other works. By these means, he had become well known to many of the learned in different parts of the world, and had been admitted a member of the Royal Societies of London and Edinburgh, therefore, a licentiate applies for an examination, a contest in reality arises between the

and several other bodies of literary men. In short, if private worth, patient industry, diversified knowledge, great general skill in medicine, and long experience of those diseases in particular, which attack Europeans in the West Indies, were qualities to be desired in a physician to his Majesty's forces there, the fitness of Dr. Wright to be one was most eminent.

To return to my narrative; in September Dr. Wright came to London, expecting to receive the promised appointment immediately upon his arrival; but he was told at the Army Medical Board, that, by a rule of Sir Lucas Pepys, it could not be given to him, *unless he had a licence to practise medicine from the college of Physicians of London.* He declared his readiness to submit to the forms necessary for obtaining one; but these could not be completed before the end of December, and the armament it was intended he should accompany was almost upon the point of sailing. Sir Lucas Pepys was therefore strongly urged by several persons to suspend his rule; among others, by two of his own friends, who told him, that Dr. Wright would certainly be appointed, whether he recommended him or not. His answer was, *he would never recommend Dr. Wright, and was sure the King would not sign his commission.* But it was quickly seen, that he had grossly overrated his consequence. It was indeed not to be supposed, that a rule of a court physician, whose connexion with the army had commenced only a year or two before, by his being placed at once at the head of its medical department, would long prevent the execution of a measure, deemed by the ablest judges highly beneficial to the military service of our country. In October, by the influence chiefly of Sir Ralph Abercrombie, Dr. Wright was appointed a physician to the armament, and shortly after went with it to the West Indies.

graduates of Oxford and Cambridge, and those of the Scotch and foreign universities. But

The only possible ground, upon which Sir Lucas Pepys could consistently with his duty to the public have formed his rule, appears to be, that he regarded an examination of medical ability by men whom he knew, and upon whose report he could therefore implicitly rely, as a necessary test of the fitness of those, who were to be entrusted with the important charge of watching over the health of his Majesty's troops. But if this be supposed the principle of his rule, what must be said of his recommending, notwithstanding, several persons to be physicians to the army, who had never undergone such an examination? Perhaps they were evidently so superior in ability to Dr. Wright, as to justify even a breach of principle in their favour:—No; they were young men, who had not yet completed their academical education, and who probably had never had the entire management of a dangerous disease committed to their care. They were, however, *Bachelors of Physic from Cambridge*.

The degree of Bachelor of Physic is now given at Oxford, the *eighth* year after matriculation; about thirty years ago it was not given till the *tenth*, but even then, so little knowledge of medicine was thought requisite for it, that he who received it was only said to be admitted, *to read the aphorisms of Hippocrates*. At Cambridge, the same degree may be obtained as soon as the *fifth* year after entrance is completed. The candidate first *keeps an act*; which consists in defending two questions, one chosen by himself, the other by the professor of medicine; but the latter is given when asked for, however long this may be before the defence is to be made. The statutes of the university require also, that the candidate should *oppose* another candidate for a degree in Physic; but this is now dispensed with *for twenty shillings*.

who are appointed to decide it? graduates of Oxford and Cambridge. The members of the

These ceremonies then have not the least resemblance to *an examination*; and no person, I believe, is ever rejected at them for want of medical learning. It is on the contrary, well known, that students at Cambridge, to save time, often take the degree of Bachelor of Medicine, when they have scarcely entered upon the study of their intended profession, meaning no doubt to apply to it with great diligence, during the *five* years which must afterwards pass away, before they can receive a *doctor's* degree. Yet, in the sight of Sir Lucas Pepys, a Cambridge bachelor of Physic appears fit, without further trial, to be a physician to his Majesty's forces in the West Indies, while a man, so gifted and adorned as Dr. Wright, appears unfit, and is therefore sent by him to be examined by the college of Physicians of London! Such are the grounds upon which the physicians of Scotch and foreign universities must build their expectations of justice from the college, when they apply for admission into the fellowship. If it be said, that no conclusion from the conduct of an individual ought to be applied to the whole body; my answer is, that the conduct of that individual must, in its principle at least, be approved by the body at large, since he is marked by their opinion to succeed Dr. Gisborne, in the presidency of the corporation.

It may be gratifying to many to know, that by his Majesty's command, orders were last year issued from the War-Office, to regulate, in future, the appointment of physicians to the army; and that, in consequence, it is now no longer necessary that they have licences from the London college, or degrees from the English universities. Those, who formerly nominated physicians to the land forces, were allowed to form their own rules, and a like indulgence was college being thus both parties and judges in the cause, it will doubtless be thought, that from respect to their own characters, they have attempted by every means in their power to lessen the invidiousness, and even danger of their situation. Have they truly done so? No, no, my Lord. They have, on the contrary, invented a mode of trial, which places their adversaries in the most difficult and humiliating circumstances, and lays themselves open to the influence of some of the basest passions of the human mind.

In the first place, before a licentiate is admissible to the examination he desires, it is demanded by the college that he be of seven years standing, and upwards of thirty-six years of age. But a rivalship for seven years with his judges, for employment, may have excited considerable animosity against him in the minds

for some years enjoyed by Sir Lucas Pepys. When this was taken away, some persons thought, that after such a disgrace, as they termed it, he would feel himself obliged as a man of spirit, to resign his office, as he could in no other way demonstrate the purity, if not the wisdom, of his intentions in framing the rules which had been annulled. Fortunately, however, he has been influenced by no such extravagant notions of personal dignity; but from unbounded zeal for his sovereign's glory, and a most tender regard for the welfare of our gallant soldiers, in *every* part of the world, still remains Physician General to the army.

of some of them; and the disgrace of being rejected at an examination must prove highly injurious, not only to the reputation, but to the fortune also of a physician, who has passed his thirty-sixth year. Such a disgrace may even more readily befal him than a younger man. For many things which he formerly learned, and the knowledge of which is required at the college examinations, are now unknown to him, from never having experienced their use in the exercise of his profession; and his present occupations may afford little leisure for regaining them.

But secondly, the application for his examination can be made upon only one day in the year, and it must not even then come directly from himself; he must find some fellow of the college to make it for him. As the number of resident fellows, however, is under thirty, it may surely happen, that they shall all agree to regard it as a point of honour not to propose a licentiate for examination.

Let it now be granted, that a fellow has proposed him; in this case your Lordship, during the trial of Dr. Stanger's cause, seemed to think, from your acquaintance with the pure and honourable conduct of the benchers of the inns of court in similar situations, that admission into the college must follow of course. But, in truth, he has only gained a title to have a vote taken by the secret method of ballot at the present meeting of the corporation, whether his qualifications for a fellowship shall hereafter be examined. If a bare majority be against his being examined, the proceedings are stopped, and cannot be begun again for a twelvemonth. I need not, however, point out to your Lordship, how much more likely it is, that a majority of votes, secretly taken, should appear against a licentiate *before* an examination, than that an English graduate should be rejected by a similar mode of voting, *after* he has been examined and approved by the president and censors, this being the only time at which the latter is subjected to a general ballot, before admission into the college.

The examination, which may have been allowed to the licentiate in consequence of the ballot, is of the same kind as that which an English graduate undergoes; but the first part of it is not held till three months after the grant, and the same space of time is interposed between its first and second parts, and between the second and third. In this way, if he is not in the mean time rejected, he is to be tortured for nine months with doubt and anxiety respecting its event. All its parts too are held, not at the private meetings of the president and censors, as in the case of an English graduate, but at the public meetings of the corporation; and should he, from natural timidity, or from that embarrassment which every man must feel, upon personally submitting his talents to the scrutiny of those, whom he believes to be unfriendly to his views, appear ignorant of any of the subjects proposed, no opportunity is allowed to him, as to an English graduate, of compensating such a seeming deficiency by any after-exhibition of knowledge. For the majority of a general meeting must declare their approbation of the first part of his examination, before he can be admitted to the second; and of the second, before he can be admitted to the third. If every part of his examination has been approved, and he has thus obtained four majorities of general meetings of the corporation in his favour, all of them declared by ballot, three months afterwards, that is, twelve months after being proposed for examination, he may be proposed at another general meeting for admission, and if the majority is found by a fifth ballot to consent, he is then to be received into the college as a fellow.

These conditions of a licentiate's entry into the college are contained, I confess, in a by-law, which your Lordship pronounced to be, not only free from blemish, but possessed of such virtue, as to render sound an older by-law, emphatically declared by you to have had in it *the seed of death*, before it received this new infusion of health. I am much inclined, however, by what has been already mentioned, to suppose, that your Lordship's opinion was derived from a very cursory view of the subject to which it relates, and I embrace this conclusion more strongly, when I consider a further point of difference between the by-law in question, and that for the admission of physicians from Oxford and Cambridge, the simplest notice of which must excite disgust and indignation in every bosom, the least animated by a love of justice.

The persons, who decide on the examination of an English graduate, are those to whom it is committed, the president and censors. The examination of a licentiate is also committed to the president and censors, but not its decision. When this is given, they vote as individuals only, in a meeting consisting frequently, I believe commonly, of more than twenty members, none of whom, except themselves, are under any other than the ordinary obligations of men to good conduct, or are even required to be present at the examination, whose event they are to determine. But if these obligations have been esteemed insufficient to ensure justice from English graduates to one of their own class, and it has therefore been thought necessary to

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delegate the decision upon his merits to five persons, who are solemnly sworn to the faithful discharge of their duty, what notion are we to entertain of the design of the college in committing the decision upon the merits of a licentiate to the *discretion of a general meeting*? We are taught, my Lord, by the slightest experience in the affairs of the world, to seek for the motives of men in their actions, when these are at variance with their words. No credit was ever given by the Romans to the declarations of clemency, with which Domitian used to preface his cruelties, or by ourselves to the robbers and murderers of France, when they pretended, that their conduct towards foreign nations arose from a disinterested desire to give liberty and happiness to mankind. When, therefore, I observe, that the college of Physicians have permitted themselves to decide upon the examinations of licentiates, without the restraint of an oath, at the same time that they strictly swear those to do justice, who are to decide upon the examinations of the graduates of Oxford and Cambridge, I hold myself fully authorised to infer, notwithstanding any protestation to the contrary, that their design in establishing this difference was, to allow room in the former set of examinations, if any such should ever take place, for the operation of principles, the most remote that can be conceived from honour and good faith.

It will perhaps be expected, that I should illustrate what I have said upon the theory of this by-law, by an appeal to the facts which have relation to it. But scarcely any such exist. During the nineteen years which intervened between the framing of the law, and the decision of the Court of King's Bench in the case of Dr. Stanger, the licentiates had been so intimidated both by its intrinsic difficulties, and by the threats of fellows of the college, that no person who applied under it should ever obtain what he desired, that only one of them, Dr. James Sims, had endeavoured to profit by it. He was regularly proposed for examination by Dr. Burges, whose motion, however, the college refused even to consider, on the ground that no one had seconded it. With what justice or decency this was done, I learn from your Lordship. "He is not to wait to be seconded," your Lordship said, in Dr. Stanger's case, while speaking of a licentiate in the situation of Dr. Sims, "the by-law does not require that." These circumstances respecting Dr. Sims were mentioned to the court by Mr. Christian, one of Dr. Stanger's counsel, but, I suppose, in a manner too unimpressive to fix them in your Lordship's mind. For had they been present to it, when your decision was given, you would necessarily have entertained some suspicion, that they, who had openly violated one part of a law, were not to be restrained by honour and good faith from violating any other part of it, when their conduct should be screened by a ballot.

The last act of the college, to which I shall solicit your Lordship's attention, seems alone sufficient to have demonstrated their total unfitness to decide between themselves and other men, when the only guard against their doing wrong should consist in their feelings of what is right. Some of the circumstances, indeed, which I am going to relate, occurred in your Lordship's presence, in the course of Dr. Stanger's cause; and I am not ignorant, that you then considered them as unconnected with any *serious* intention, on the part of the college. Admitting, however, for a moment, this to have been the case, surely the system of morality, which permits its followers to accuse a gentleman, by way of joke, of a most disgraceful crime before the Lord Chief Justice of England, ought to have no place in *the sanctuary of honour and good faith.* But not to dwell longer upon this argument, I shall, I think, soon convince your Lordship, that the charge to which I have alluded was deliberately formed, and seriously urged by the college, with the horrible design of destroying the character of an innocent person, because he was bold enough to oppose their injustice.

When a physician is admitted by the college into the class of licentiates, he gives his promise or faith, that he will observe their statutes, *or* readily pay the fines which shall be imposed upon him for disobedience*. Sir William Blackstone, who, I believe, is not generally reckoned a loose moralist, holds it established, that, when a penalty is annexed to the non-compliance with laws, "which enjoin only *positive* duties, and forbid only such things as are not *mala in se*, but *mala prohibita* merely, without any intermixture of moral guilt—the alternative is offered to every man, 'either abstain from this, or submit to such a penalty;' and [that] his conscience will be clear, whichever side of the alternative he thinks proper to embrace." Possibly some doubt may be entertained of the justness of this doctrine when applied to laws, which affect all persons equally, and are made by those who are to be controlled

* The president says to him—dabis fidem, te observaturum statuta collegii, *aut* multas tibi contrà facienti irrogandas promptè persoluturum, omniaque in arte medica pro viribus facturum in honorem collegii, et reipublicæ utilitatem—to which he assents.

by them. But, however this may be, it is at least certain, that no doubt can exist, whether a licentiate is entitled to take either side he pleases of the alternative, which is offered to him by the college themselves, not by implication, but by the most direct and explicit expression, with respect to the observance of statutes, made always without his consent, and sometimes with the avowed design of placing him beneath men, whom the laws of their common country declare to be no more than his equals. He will even merit no blame from them, as lawgivers, by disobeying such of their statutes as forbid what is evil in itself, provided he immediately pays the fines which are demanded from him. The blame, which he here incurs, depends upon his having broken the laws of some far higher power, those of God or his country. But I fear I render this subject confused, by holding it up too long to view. Luminous objects are best discerned by a single glance of the eye; if we suffer our sight to dwell upon them, their very brightness soon causes them to appear indistinct.

The degree of obedience, which is due by a licentiate to the laws of the college, being then so evident, no one can imagine, that it was ever unknown to the many learned and well-informed men, who are members of that body. The intention, therefore, of those men, in acting even for the shortest time, as if it were unknown to them, could not have been *honourable*; but as they persisted in this conduct for nearly three years, they must necessarily have been *serious*. Shortness of duration is essential to every kind of joke, whether verbal or practical.

About the middle of 1794, a rumour became prevalent among medical men in London, that the college viewed, as a breach of faith to them, the attempt of certain licentiates to render the corporate distinctions of their profession accessible to every physician of sound morals and learning; but it was thought by those licentiates too absurd to be credited. "We know," said they, "of no statute of the college, by which we are forbidden to endeavour to gain admission into it. If there be any such, let it be pointed out, and let the fine be demanded, which is annexed to our disobedience. Were indeed such a statute to exist, it would be not only tyrannical, but contrary to the laws of our country, and therefore without force. At all events, to desire the removal of a grievance can never be justly held a breach of our promise to the college. For to what purpose has the Court of King's Bench been charged with the inspection and controul of corporations, if applications to it against the oppression of by-laws can, by other by-laws, be legally declared violations of faith in those who seek for relief?" But they soon discovered their mistake in supposing that the rumour must be false, because it seemed to them absurd; for in October of the same year, the accusation which it contained was publicly brought against them by Dr. John Latham, one of the fellows of the college. "We are attacked*," said Dr. Latham in his Harveïan oration, "by ferocious, daring, and obstinate enemies, regardless of the faith which they have pledged for the observance of our statutes.—— I might complain at greater length of the injury which they have rashly done us, but *liberality* forbids me to say more."

Flagitious conduct, my Lord, ought, in my poor opinion, never to pass uncalled by its proper name. If vice be not termed vice, if baseness and dishonour be suffered to come forth into the world, without the mark of infamy, we remove one of the most powerful checks upon the evil inclinations of man, and

* "Hostis—aggreditur, ferox, audax, pertinax, posthabita fide de observandis [collegii] statutis.—Verum enimvero tametsi mihi esset occasio querendi prolixius de facta nobis temere injuria, vetat amplius disserere liberalitas." These quotations are from the printed copy. The author of this letter did not hear Dr. Latham deliver his oration, but from the reports of others he has reason to believe, that the whole of the abuse, which was then thrown upon the associated licentiates, has not been printed.

indirectly discourage the practice of virtue. If, therefore, the titles of reproach used by Dr. Latham had been merited, it would have been gallant, it would have been praiseworthy in him to have bestowed them. But to whom were they applied? To fourteen persons of his own profession, all of whom, except one, were at least equal to himself in every quality and accomplishment, which physicians are required to possess. And upon what occasion? Because these men had, in a temperate, and even respectful address to the college, set forth their claims to admission into the fellowship, and had requested to know, whether they would be allowed to prove their fitness for what they desired, by undergoing the examinations which are prescribed for the graduates of Oxford and Cambridge. This was the only measure they had hitherto taken for obtaining their object. Your Lordship will now assuredly conceive, that such expressions were heard with disgust by the other members of the college. I firmly believe, my Lord, that they were heard with great disgust by some of its members. But the body at large hastened to adopt them, by soliciting their author to print his oration. Happy, however, would it have been for Dr. Latham, if their zeal to injure the moral characters of those, whom they denominated their enemies, had not blinded them to the danger, to which they were about to expose the literary reputation of one of their dearest friends; if they had not by their own praises so fanned his desire for general applause, as to occasion his giving a work to the world, which sets at defiance every principle of taste in composition, and exhibits more than a schoolboy's ignorance of the common language of the learned.

The next public indication of the plan of the college to defame the associated licentiates, (for I purposely avoid mentioning any private proof of it) was furnished in April 1796, by Sir George Baker's swearing before your Lordship, that Dr. Stanger, upon being made a licentiate, had given his faith, or promise, that he would obey their statutes. It now became clearly evident to those, who had watched the conduct of the college, that they meant to urge this, among other arguments against the claim of that gentleman, that he was unworthy of admission into their body, from having, by his present application to the court, forfeited all title to confidence in his future declarations. No notice, indeed, was taken of this part of Sir George Baker's affidavit, in the pleadings which immediately followed; but Mr. Erskine was the only one of their counsel, who completed his speech upon that occasion, and there are strong grounds for concluding, (with which, however, I shall not trouble your Lordship,) that his omitting to bring it forward was highly disagreeable to his employers.

In January, 1797, the circumstance of Dr. Stanger's having given his faith to observe the statutes of the college was a second time sworn to by their president, and in the trial which took place in May, Mr. Erskine did not again neglect to touch upon it. But the whole of this part of his speech seemed to denote a struggle between the ingenuous feelings of a gentleman, and the desire of an advocate to gratify his clients. "I do not mean to say any thing offensive to Dr. Stanger; he will understand that I am using the words of Lord Mansfield. —I have done justice to this gentleman, who, I have no doubt, is a learned man, and a person of honour and character in his profession." These were expressions employed by Mr. Erskine, while speaking of the engagement under consideration. But as the only possible view of the college, in producing it to the court, must have been to pretend that it had been violated, to call Dr. Stanger "a person of honour" was directly in opposition to their design, and plainly demonstrated the aversion of their principal advocate to lend his aid towards its completion.

The two advocates of the college, who spoke next, were silent upon the subject of Dr. Stanger's engagement. But their deficiency on this point was fully supplied by the youngest counsel, Mr. Warren. He was the son of one of his employers, and consequently possessed the most ample opportunities of being acquainted with their real motives and views, and as he had evidently been retained in the present cause, for reasons unconnected with his general fame, he must have been strongly disposed to requite the favour he had received, by doing what he knew would be most agreeable to them. *He* therefore did not inform the court, as Mr. Erskine had done, that he was not instructed to make any insinuation against the character of Dr. Stanger, but boldly and explicitly charged that gentleman, with "a violation of something, less formal, but not less sacred, than an oath." The court now exerted their authority, and prevented his proceeding further in this strain*. But, my Lord, had the dagger,

* My authority for saying, that Mr. Warren was interrupted in this part of his speech, is the following conversation between Lord Kenyon and Mr. Christian, one of Dr. Stanger's counsel, which took place two days after, while the latter was replying to the arguments against the issuing of the mandamus.

Mr. Christian. "An argument was pressed the other day which he drew from beneath a robe, intended to give dignity to the assertor of innocence and right, been even suffered to reach its destined object with all the force that his arm could impart, it would have still struck harmless upon the armour of honourable reputation, to the

which I was sorry to hear, because it might wound the feelings of a very honourable mind; it was said that Dr. Stanger had pledged his faith to observe the statutes."

Lord Kenyon. "That was put an end to immediately as it was mentioned."

Mr. Christian. "It seemed to be pressed and relied upon as a serious argument."

Lord Kenyon. "Certainly not."

I must, however, confess, that I see no mark of any such interruption, in Mr. Gurney's report of Mr. Warren's speech. I presume, therefore, that the Court's disapprobation of the shameful attack upon Dr. Stanger's character must have been expressed by some gesture or look from the Bench, which, though sufficiently intelligible to Mr. Warren, might easily pass unobserved by a writer intent upon his papers. How far his Lordship himself thought the honour of that physician affected by his application to the court, may be known from the following passage in his speech at the close of the trial. "It is fit that I should put the mind of Dr. Stanger, in case it is in an uneasy situation, in a perfect state of repose with regard to one thing. Undoubtedly his moral character is not at all tainted by the application that is now made. I have not the honour of knowing him; I have heard nothing but to his advantage when I have heard him spoken of, and I dare say all the eulogy, which his warmest friends could bestow upon him, his character both as a moral and professional man deserves."

confusion of every hope conceived by the cold-blooded, corporate cruelty, which had urged him to the deed.

I cannot forbear making one observation more upon this atrocious attempt of the college. Though a licentiate is obliged to give his faith, that he will observe their statutes, he is never furnished with any opportunity of learning what they are. The last printed edition of them is dated in 1765, and is now so scarce, that many, I believe I may justly say most, of the licentiates have never seen a copy of it. The code too, since 1765, has undergone very considerable alterations, none of which, as far as I know, have ever been communicated to the licentiates. In 1796, Dr. Stanger swore before the Court of King's Bench, that to the best of his knowledge and belief, no person could be admitted into the order of candidates, who did not enjoy, by birth, all the privileges of a British subject; and yet it was afterwards declared by the counsel of the college, that the statute requiring this condition had been repealed upwards of twenty years. Dr. Stanger swore also, that he had shortly before applied to the president and register of the college, for some information respecting their laws, but that both those officers had refused to give it to him. Caligula, among other acts of tyranny, caused several of his edicts to be written in very small letters, and afterwards fixed in situations of difficult access, in order that those who were to be affected by them might offend through ignorance. His ultimate object, however, was only to procure the pecuniary fines which were imposed upon the want of obedience; when these were obtained, he readily acquitted the transgressors of all further blame. Men calling themselves Britons likewise conceal their laws, but, with a refinement in cruelty beyond the conception even of a Roman tyrant, declare persons to be infamous, who do not observe them.

I have now, my Lord, finished my journey through the dreary waste, which I undertook to explore. In my progress, no spot of verdure has been found, upon which the wearied eye might repose, and scarcely an object of terror has occurred, to break the flat uniformity of the scene, one wide expanse of pitiful fraud, and paltry chicane. My labour has been inglorious; but should it furnish your Lordship with a more accurate knowledge of the ground I have passed over, than that which you formerly possessed, I shall esteem it most amply repaid.

That the conduct which I have described

should have been exhibited by men, many, perhaps all, of whom discharge with propriety the duties of their private stations in society, is one of those facts relative to the human character, which, however difficult to be explained, are still unquestionably true. There is a certain gallantry in doing a wrong thing for the sake of another, which in some degree lessens the deformity of the action. The odiousness of such an action is still further diminished, should it tend to the benefit of many. If it promises to promote the interests or happiness of a whole nation, its name, if not its nature, is often changed; and what in private life would have been denominated vicious, may now be regarded not only as pardonable, but even as meritorious. Besides; the members of corporations commonly imagine, that they have a right to do every thing which has been done by their predecessors, notwithstanding the circumstances may have long ceased to exist, under which their ancient rules were established. Again; the actions of most persons, when they are not under the dread of general laws, seem to be chiefly regulated by the praise and blame of those by whom they are immediately surrounded. The peasantry upon our coasts, who in the ordinary situations of life do not appear to be more depraved than other men, have often been known to commit, in bodies, the most detestable cruelties upon shipwrecked mariners; and the vilest malefactors often meet death at the gallows with the greatest firmness, if strengthened by the presence and approbation of their former companions. If to such considerations we add, that no one is personally answerable for the acts of a corporation, and that these often proceed from a bare majority, or a number even less than a majority of its members, we may possibly obtain from the whole an explanation, why the public conduct of the college of Physicians is frequently so very different from what any one might expect, who has looked only to the private characters of some of those who compose it. But whatever opinion may be formed concerning the grounds of explanation which I have offered, the fact, to which they are meant to apply, still rests upon the basis of testimony, and is laterally supported by innumerable other facts of the same kind. "All men," said an author, whose wisdom and eloquence have produced a change in the state of human affairs scarcely inferior to any, that has ever been effected by the arms of a conqueror, but who most unfortunately does not live to witness the gratitude of the world, for his noble, energetic, and invigorating exhortations to resistance against its common and most dangerous enemy, when almost every one was benumbed by despair, and sought only to prolong a miserable existence by base submission; "all men," said Mr. Burke, "possessed of an uncontrolled discretionary power, leading to the aggrandizement and profit of their own body, have always abused it; and I see no particular sanctity in our own times, that is at all likely, by a miraculous operation, to over-rule the course of nature." I have thought proper to add thus much, to free myself from the suspicion of being actuated, in what I have written, by private resentments against individual members of the college. If such feelings had ever been produced in me, it would have become my duty, and I trust I should have had strength to perform it, either to stifle them as unworthy of life, or to make known their existence, in a more direct way than the present, to those who had given them birth.

A more difficult task, my Lord, remains for me to perform—that of again apologising to you for this letter. When I began it, my only view was to acquaint your Lordship with the event of an application to the college of Physicians, which had been occasioned by your advice. But, after I had proceeded some way in accomplishing this design, I thought it might be both curious and useful to show, that their rejection of the application was not inconsistent, either with the principles which it might have been supposed would influence a body of physicians in their situation, or with the actual tenour of their conduct, prior to the decision of the Court of King's Bench in the case of Dr. Stanger. I saw, indeed, that such an attempt would be an indirect attack upon the propriety of that decision, not as connected with the intentions of those who gave it; the honour, and integrity, and uprightness of English judges, like axioms in science, are always beyond doubt; but as far as it was founded in considerations, the strength, or weakness of which many persons had better opportunities of knowing than your Lordship or brethren. I imagined, therefore, that in making the attempt, I should only act similarly to one, who applies to a court of justice for a new trial of his cause, in consequence of obtaining new evidence to support it, or who appeals from the jurisdiction of one court to that of another; and hence I concluded with some confidence, that the *plan* of my letter would be regarded by your Lordship as blameless. But now that it is finished, I greatly fear, that the *execution* will not be esteemed altogether so; that, on the contrary, the liberties of expression in which I have sometimes indulged may appear to your Lordship, if indeed you should ever bestow a moment's thought upon the subject, as not a little reprehensible.

The plainness and freedom of speech, my Lord, which so remarkably distinguish Englishmen, have always seemed to me, not only to be essentially connected with the existence of their thrice happy and unparalleled form of government, but even to give rise, in great measure, to some of their characteristic virtues; among others, to their humanity. I mean not the humanity which is dictated by policy, or that which originates in a morbid sensibility incapable of bearing the sight of distress; but the humanity which is so firmly ingrafted upon the wild stock of our populace, that the greatest storms cannot tear it away; the humanity which withholds our mobs, in their most guilty excesses, and while maddened by strong liquors, from the spilling of blood. Hatred and revenge spring up in concealment, and must be nourished by long and painful meditation upon injuries received, before they can attain any vigour. But Englishmen, by loudly and fearlessly declaring their wrongs as soon as they feel themselves aggrieved, prevent the very beginnings of those baleful passions, and thus preserve their hearts always in a condition to obey the great command of their Maker, to venerate his image in man. Our climate, my Lord, may be rude and boisterous, but still it is free from the hurricanes, which desolate countries possessing skies, for the most part, calm and serene. Under the influence of these opinions, I have long been accustomed to give free expression to my sentiments upon the conduct of other men, and experience of the benefit hence derived to the health of my mind has contributed to establish the practice. If, therefore, I should be regarded by your Lordship as having employed too great liberty of speech in this address, I humbly request that you will ascribe my fault, either to error of principle, or inveteracy of habit, but in no degree to any deficiency of respect for your high station and character.

I retire at length, my Lord, from your presence, and at the same time relinquish my struggle with the college of Physicians. I consider myself now as a veteran in the contest, and therefore as entitled to repose;

Spectatum satis, et jam donatum rude.

To those, however, who still combat on the side which I have endeavoured to support, I venture to address myself, though without the smallest pretension to be a leader of men, yet in the language of one,

——— μήπω τι μεθίετε θούριδος ἀλκῆς·
Οὐ γὰρ ἐπὶ ψεύδεσσι πατὴρ Ζεὺς ἔσσετ' ἀρωγός.

I have the honour to be,

My Lord,

Your Lordship's most obedient,

And most humble Servant,

WILLIAM CHARLES WELLS.

London,
July 1, 1799.

AN

ACCOUNT

OF A FEMALE

OF THE WHITE RACE OF MANKIND,

PART OF WHOSE SKIN

RESEMBLES THAT OF A NEGRO;

WITH

SOME OBSERVATIONS

ON THE

CAUSES OF THE DIFFERENCES IN COLOUR AND FORM

BETWEEN

THE WHITE AND NEGRO RACES OF MEN.

ACCOUNT

OF A FEMALE

OF THE WHITE RACE OF MANKIND,

PART OF WHOSE SKIN

RESEMBLES THAT OF A NEGRO, &c.

INSTANCES of the absence of the black colour, in the whole or part of the skin in persons of the negro race, are not very uncommon; but there is, I believe, no one upon record of an individual of the white race having any part of the body, covered with a skin similar to that of a negro. The following account, therefore, of such an instance, will, perhaps, be acceptable to the philosophical public. I have been enabled to form it by the permission of Dr. Turner, one of my colleagues at St. Thomas's Hospital, into which the person, whose case I am to describe, was lately admitted by him, on account of some bodily ailment.

Hannah West, the subject of this account, was born in a village in Sussex, about three miles distant from the sea, and is now in the twenty-third year of her age. Both of her parents were natives of the same county. Her father was a footman in a gentleman's family, and died while she was very young. She cannot, therefore, remember his appearance; but she has never heard, that it was in any way extraordinary. Her mother is still alive, and has black hair and hazel eyes, but a fair skin, without any stain or mark upon it. West was the only child of her father; but her mother, having married a second time, has had eleven other children. Nine of these are living, all of whom are without any blackness of the skin. Her mother, she says, received a fright, while pregnant with her, by accidentally treading on a live lobster; and to this was attributed the blackness of part of her skin, which was observed at her birth.

West is somewhat above the middle size, is rather of a full habit, and till she came to London from Sussex, which was about four months ago, always enjoyed very good health. The hair of her head is of a light brown colour, and is very soft; her eyes of a faint blue; her nose prominent and a little aquiline; her lips thin; the skin of the greater portion of the uncovered parts of her body very white; in short, her appearance is in every respect, except the one which has been mentioned, that of a very fair female of the white race of mankind.

The parts covered by the black skin are, the left shoulder, arm, fore-arm, and hand. All these parts, however, are not universally black; for on the outside of the fore-arm, a little below the elbow, a stripe of white skin commences, about two inches in breadth, and differing in no circumstance from the skin of the other arm, which, proceeding upwards, gradually bends under the arm, and at the arm-pit joins with the white skin of the trunk of the body. The black skin, wherever it is contiguous to the white, terminates rather abruptly, so that its boundary may always be distinctly traced.

The colour of the black skin is not every where uniformly dark. Thus, the skin of the back of the hand, and of the wrist, is marked by fine lines of a reddish black, which cross one another at right angles, while the small rectangular spaces bounded by these lines are entirely black. Part of the cuticle of the hand having been removed by exciting a blister, the reddish lines were found to be the summits of very thin folds of the true skin, which were raised above its general level, and were less thickly covered with the black *rete mucosum* than the more depressed parts. Their reddish colour was, no doubt, occasioned by the external air, as the skin of the other hand was red from that cause. All the other parts of the black skin are fully as dark, as I found on making the comparison, as the corresponding parts of a dark negro, and are much darker than those of many negroes. One part, indeed, of her skin is considerably darker than the corresponding part in any negro whom I have seen; for the palm of her hand and inside of her fingers are black, whereas these parts in a negro are only of a tawny hue.

A considerable part of the black skin is as smooth to the touch, as the skin of the white arm; but the cuticular lines in the black arm, appeared everywhere stronger to the sight, than similar lines in the arm of a black man, whose skin I examined at the same time. In the greater part, however, of West's black skin, those lines sink deeper beneath its general surface, than the lines of any other human skin that I have seen, which was not evidently diseased. These depressions are extremely narrow, and proceed chiefly in one direction, obliquely upwards and inwards from the outer part of the arm. On removing a small portion of the cuticle, they were found to be occasioned by the sinking down of that membrane between very narrow and slightly elevated folds of the true skin, nearly contiguous to one another, which held the direction mentioned.

A great part of the black shoulder exhibits a singular appearance; for, near to the back bone, the skin, over an extent of six inches in length and two in breadth, resembles a thick coat of pitch, or black paint, which by drying had split into a great number of small square portions. The fissures in the skin are about a line in depth. Mr. James Wilson, teacher of anatomy, and fellow of the Royal Society, who saw this person once along with me, pulled away a little of this black matter, upon which several narrow processes of the skin, perpendicular to the plane of the part, became visible.

Winslow says, that the cuticle of a negro is black, and that the contrary supposition arose from its tenuity and transparency, in like manner as a thin film of black horn appears almost colourless. I have found by my own observations, that this opinion of Winslow is just; and I found also, that the cuticle of West's black skin is likewise dark. I may add, that the nails of her black fingers are darker than those of the white, and darker also than those of a negro's hand.

Sir Everard Home, who likewise saw this person once along with me, thought that the black arm smelt more strongly than the white. I made the experiment immediately after him, and thought so too. But on repeating it several times with more attention, I could perceive no difference. It seems to me, indeed, from a similar experiment made on the arm of a dark negro, whose appearance did not lead me to suppose, that he had been very careful with respect to the cleanliness of his person, either that all negroes do not possess a strong smell, or that this does not proceed from all parts of their skin, since I could perceive no difference between the odour of his arm, and that of the white arm of West.

On the black fore-arm are about a dozen small hard substances, the largest of which are of the size of a common pea. Some of them are very black; others are less black, and one or two are of a reddish black colour. I thought, at first, that they consisted of thickened cuticle, but I found afterwards, that they readily bled upon being punctured with a needle.

The upper and outer part of the black arm has a number of very black hairs growing from it, some of which are three quarters of an inch long. The inner part of the arm, which is equally black, is free from hairs.

The black arm is as firm to the touch, and as fleshy as the white; and according to the young woman's own report, there is no difference in their strength or feelings of any kind.

The last circumstance which I shall mention concerning her is, that no change has taken place within her remembrance, either in the degree or extent of the blackness of her skin.

Two inferences may, I think, be made from what has been related respecting Hannah West.

The first is, that the blackness of the skin in negroes is no proof of their forming a different species of men from the white race.

When a white man is much exposed to the action of the sun, his skin becomes more or less brown, and as the intensity of this colour, after equal degrees of exposure, is generally proportional to the heat of the climate, it has hence been supposed, that the colour of negroes is derived from a very great degree of the same cause. But this conclusion seems to me very faulty. For, setting aside that a white man, rendered brown by the sun's rays, begets as white children as those of another of the same race, the colour of whose skin had never been altered, it appears to me probable, from observations lately made on two negroes, that the action of the sun tends rather to diminish than augment the colour of their race. Both of those persons were born in European settlements, and had been accustomed to have their bodies clothed, yet, in both, the trunk, arms, and lower extremities, were considerably darker than the face, and in one, were somewhat darker than the hands. But admitting this observation to be of no force, still it must be granted, in consequence of what has been said upon the state of part of West's skin,—that great heat is not indispensably necessary to render the human colour black; which is the second conclusion to be drawn from the account which has been given of her.

On considering the difference of colour between Europeans and Africans, a view has occurred to me of this subject, which has not been given by any author, whose works have fallen into my hands. I shall, therefore, venture to mention it here, though at the hazard of its being thought rather fanciful than just.

There is no circumstance, perhaps, in which these two races differ so much, as in their capacity to bear, with impunity, the action of the causes of many diseases. The fatality to Europeans of the climate of the middle parts of Africa, which are, however, inhabited by negroes without injury to their health, is well known. Let it then be supposed, that any number of Europeans were to be sent to that country, and that they were to subsist themselves by their bodily labour; it seems certain, that the whole colony would soon become extinct. On the other hand, the greater liability of negroes in Europe to be attacked with fatal diseases is equally well established. If, therefore, a colony of the former race were brought to Europe, and forced to labour in the open air for their subsistence, many of them would quickly die, and the remainder, from their inability to make great bodily exertions in cold weather, and their being frequently diseased, would be prevented from working an equal number of days in the year with the whites. The consequence would be, that without taking farther into account the unfriendliness of the climate to them, their gains would be inadequate to the maintenance of themselves and their families. They would thence become feeble, and be rendered still more incapable of supporting life by their labour. In the mean time, their children would die from want, or diseases induced by deficient or improper nourishment, and in this way, a colony of the negro race in a cold country would quickly cease to exist.

This difference in the capacity of the two races to resist the operation of the causes of many diseases, I assume as a fact, though I am utterly unable to explain it. I do not, however, suppose, that their different susceptibility of diseases depends, properly, on their difference of colour. On the contrary, I think it probable, that this is only a sign of some difference in them, which, though strongly manifested by its effects in life, is yet too subtle to be discovered by an anatomist after death; in like manner as a human body, which is incapable of receiving the small-pox, differs in no observable thing from another, which is still liable to be affected with that disease.

Regarding then as certain, that the negro race are better fitted to resist the attacks of the diseases of hot climates than the white, it is reasonable to infer, that those, who only approach the black race, will be likewise better fitted to do so, than others who are entirely white. This is, in fact, found to be true, with regard to the mixture of the two races; since mulattoes are much more healthy in hot climates than whites. But amongst men, as well as among other animals, varieties of a greater or less magnitude are constantly occurring. In a civilized country, which has been long peopled, those varieties, for the most part, quickly disappear, from the intermarriages of different families. Thus, if a very tall man be produced, he very commonly marries a woman much less than himself, and their progeny scarcely differs in size from their countrymen. In districts, however, of very small extent, and having little intercourse with other countries, an accidental difference in the appearance of the inhabitants will often descend to their late posterity. The clan of the Macras, for instance, possess both sides of Loch-Duich in Scotland; but those who inhabit one side of the loch are called the black Macras, and the others the white, from a difference which has always been observed in their complexions. Again, those who attend to the improvement of domestic animals, when they find individuals possessing, in a greater degree than common, the qualities they desire, couple a male and female of these together, then take the best of their offspring as a new stock, and in this way proceed, till they approach as near the point in view, as the nature of things will permit. But, what is here done by art, seems to be done, with equal efficacy, though more slowly, by nature, in the formation of varieties of mankind, fitted for the country which they inhabit. Of the accidental varieties of man, which would occur among the first few and scattered inhabitants of the middle regions of Africa, some one would be better fitted than the others to bear the diseases of the country. This race would consequently multiply, while the others would decrease, not only from their inability to sustain the attacks of disease, but from their incapacity of contending with their more vigorous neighbours. The colour of this vigorous race I take for granted, from what has been already said, would be dark. But the same disposition to form varieties still existing, a darker and a darker race would in the course of time occur, and as the darkest would be the best fitted for the climate, this would at length become the most prevalent, if not the only race, in the particular country in which it had originated.

In like manner, that part of the original stock of the human race, which proceeded to the colder regions of the earth, would in process of time become white, if they were not originally so, from persons of this colour being better fitted to resist the diseases of such climates, than others of a dark skin.

The cause which I have stated, as likely to have influence on the colour of the human race, would necessarily operate chiefly during its infancy, when a few wandering savages, from ignorance and improvidence, must have found it difficult to subsist throughout the various seasons of the year, even in countries the most favourable to their health. But, when men have acquired the knowledge of agriculture, and other arts, and in consequence adopt a more refined mode of life, it has been found, that an adherence to their ancient customs and practices will preserve them long as a distinct race from the original inhabitants of the country to which they had emigrated. Examples of this kind are frequent in the islands in the eastern seas in the torrid zone, where the inhabitants of the sea-coast, evidently strangers, are in some degree polished, and of a brown colour, while the ancient natives, who live in the interior parts, are savage and black. Similar facts occur in respect to other species of animals. It seems certain, for instance, that fine woolled sheep, like the Spanish, never both arose and sustained their breed in the northern parts of Europe; yet, by care, this feeble race, after being formed in Spain, has been propagated and preserved in very cold countries. Thus the late Mr. Dryander, the learned librarian of the Royal Society, informed me, that the breed of fine woolled Spanish sheep had been kept perfect in Sweden during a very long term of years, I think he said a century. If, then, my memory be accurate upon this point, we have here an example of a variety of animals, much more liable to be affected by external circumstances than the human race, being preserved without change, in a country very different from their own, by assimilating their new state as much as possible to their old, during at least fifty generations, that is, during a period equivalent to 1500 years in the history of man.

Hitherto, while speaking of the external appearance of negroes, I have taken notice only of their colour. I shall now say a few words upon their woolly hair, and, according to our notions of beauty, the deformity of their features.

There are several facts which seem to show, that these circumstances are somehow connected with their low state of civilization.

First; the black inhabitants of the Indian Peninsula within the Ganges, who, compared with the African negroes, may be regarded as a polished people, have hair and features much less dissimilar to the European.

Secondly; Woolly heads, and deformed features, appear again, as we proceed further to the east, among the savage inhabitants of New Guinea, and the adjacent islands, at the distance nearly of half of the circumference of our globe from Africa, and consequently without the smallest probability of any communication having ever existed between the two countries.

Lastly; it appears probable from the reliques of ancient art, that the early inhabitants of Egypt were of the negro race. If, then, the negroes of Africa were ever to be civilized, their woolly hair and deformed features would, perhaps, in a long series of years, like those of the Egyptians, be changed. On the other hand, their present external appearance may possibly be regarded not only as a sign, but as a cause of their degraded condition, by preventing, in some unknown way, the proper developement of their mental faculties; for the African negroes have in all ages been slaves; and the negroes in the eastern seas are in no instance, I believe, masters of their handsomer neighbours, but are in many places in entire subjection to them, though the latter be frequently less numerous.

It will no doubt be objected to what I have advanced respecting the difference of colour between Europeans and Africans, that the Indian inhabitants of the greater part of the immense continent of America have skins nearly of one hue. Plausible reasons may, I think, be given for this fact, consistently with what has been said upon the colour of the two former races; but I forbear trespassing any longer upon the time of the reader, in discussing a subject which admits only of conjectural reasoning.

THE END.

LONDON:
T. DAVISON, LOMBARD-STREET, WHITEFRIARS.